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TWO EVOLUTIONARY THEORIES (II) *

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8 *A Choice of Two Closed Systems ?*

There are, then, two ways of looking at the evolutionary record. The disagreement between the two is not, chiefly, about verifiable or falsifiable matters of fact, but about the concepts through which these facts are to be interpreted. Each constitutes, for its proponent, a closed interpretive system *in* which he sees the facts. In such a case, the objections of one to the arguments of the other are, from the other point of view, irrelevant. Can we weigh two such systems, cognitively, at all ? Or are they like an ambiguous figure which we could see one way or the other, so that we may simply take our choice ? In a sense, they are: for reflecting on the evolutionary record, as biologists agree in telling us about it, we may think of it in either of these ways.[1]

Thus we may seek to lose ourselves in what Simpson describes as ' great groups of animals living their history in nature '.[2] Or we may look rather at the ascending levels of organisation into which the continuous stream of protoplasm has shaped itself. So far, then, the two theories do seem to provide alternative frameworks for understanding the data, and it seems to be purely a matter of choice which we prefer. With respect to some details within each theory, this is in fact clearly the case. For example, it is really a matter of choice whether we say that the higher categories come before the lower, or conversely, the lower before the higher. In fact, each of our two scientists, in this connection, explicitly concedes the other's position,

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* Part I of this paper appeared in the August number

[1] We may also, of course, think of it in other ways; I am not suggesting that these are the only two; they are the two I have been looking at in this study.

[2] *Major Features of Evolution*, p. 265

Simpson admits that *Archaeopteryx* was already a bird,[1] and Schindewolf admits that though *Archaeopteryx* was definitely a bird, if there had never been any more birds nobody would have known it.[2] Thus, on this particular point, each system can comfortably assimilate a theorem from the other, without endangering its total structure. So far as this particular case goes, we really can look at the matter either way.

For some details, on the other hand, one point of view seems preferable, while the other fits other cases better. Looking at the two, we may find, in this case, the choice more difficult. For example, the gradual change from one extinct species of elephant to another (marked by an increase in lamellar index, i.e. in the number of enamel plates per 10 mm. of tooth) is well handled by Simpson's purely phylogenetic taxonomy.[3] It is difficult to see, on the other hand, how the origin of mammals could be handled in these terms.[4] Perhaps what we need, then, is a more inclusive theory, which will assimilate adequately both sides of the ambiguity. What would be the requirements of such a theory ? This is a difficult question, and one with grave philosophical implications; I can only suggest here a starting point for a possible answer.

9 *Philosophical reflections*

As philosophers reflecting on scientific explanation, in this case on theories of evolution, we have ourselves to stand somewhere. We have to formulate our reflections within a framework of our own. I would specify, in my own reflection on these or any other evolutionary theories, three criteria, one methodological and two ontological, in relation to which, I believe, such a theory should be assessed.

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[1] That is, it had feathers; but was 'as reptilian as avian throughout' (ibid. p. 370). For a more general 'concession' on higher categories, see page 350: 'In these usual cases it is true that occupation of the zone, which in retrospect is the origin of the higher category, precedes the origin of numerous genera, species and other units that come to comprise the higher category. In *this sense*, and this only, we can agree with Wright . . . that " there seems to be a large measure of truth in the contention of Willis and Goldschmidt (also Schindewolf, G. G. S.) that evolution works down from the higher categories to the lower rather than the reverse ".'

[2] *Grundfragen der Paläontologie*, p. 453

[3] *Major Features of Evolution*, pp. 387-388

[4] See e.g. *Grundfragen der Paläontologie*, pp. 250-251. See also the papers by Dalcq referred to in n. 1, p. 123

(*a*) *Methodological.* Each of our two palaeontologists, we found, accused the other of similar fallacies; and in particular each accused the other of arguing in retrospect. This, I think, gives us a clue to the necessary method in evolutionary theory. For this is where they are both wrong, or both right, if you prefer. Each one does indeed argue in retrospect but this in itself is not a fallacy. For what else could they do ? Thucydides could not have written the *Decline and Fall of the Roman Empire*, nor could a pre-Cambrian organism, or almost-organism, have stated a theory of evolution. This is not just trivially true. The judgment of the scientist is an essential element in every scientific explanation, and especially so in a theory of evolution, since here the scientist is judging not simply the world but the world including himself and his kind, *as* the world that produced himself and his kind. The French zoologist Vandel has formulated this principle for evolutionary theory as the ' Method of Recurrence '.[1] It is nonsense, he says, to decry anthropocentrism in evolution; there is nowhere to start except where we are. This is, I believe, an essential principle for evolutionary theory, as for all historical disciplines. There is no such thing as total objectivity in historical statements; we can only look back and evaluate the evidence from where we are. All historical discourse, including evolutionary theory, is in this sense reflective. In evolutionary theory, moreover, it is ourselves, including our capacity for historical reflection, that we have to explain.

(*b*) *Ontological.* Looking back from where we stand, therefore, at the vista of evolution, what is it that we see ? Here, I think, both in the light of the biological evidence (as a layman, and as some biologists see it), and of everyday experience, we ought to admit two principles which should *both* be taken just account of in evolutionary theory. First, we should acknowledge the continuity of life, and for that matter, once embarked on an evolutionary retrospect, of life with matter. Secondly, we ought also to acknowledge the diversity of ordering principles which appear at numerous levels in this continuous process. New principles of organisation appear to have arisen out of conditions that in themselves stretch back continuously—yet they are principles of organisation which order the conditioning continuum in a genuinely new way.[1] This duality, whatever the difficulties of stating it precisely, is essential in some form to any adequate assessment of the achievement of evolution.

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[1] A. Vandel, *L'Homme et l'évolution*, Paris, 1949 (2nd ed. 1958). Vandel's Method of Recurrence is, in effect, an application to the problem of evolution of Polanyi's ' fiduciary programme '; see M. Polanyi, op. cit. pp. 264-268, and cf. also the argument on evolution in Part IV, Chapter 13 of the same work, pp. 381-405

Now with respect to this duality, I submit, our two theories are in a rather different position. Therefore, as philosophers reflecting on evolutionary theory, we can and should allow them a different weight.

The dynamic of neo-Darwinism, I said earlier, is towards the denial of structure in the sense of reducing it to its conditions. The aim of Darwinism was to shake life out of its rigidity and thus to set it free. But this liberation obeyed also other passions of the modern mind: the reduction of principles to the least possible number, the love of explanation in terms of the least common denominator, of parts rather than wholes, conditions rather than reasons. Life is all matter, *only* organised differently.[2] Even those thinkers, oddly enough, who have stressed the term ' emergence ', Lloyd-Morgan and Alexander, have insisted on weighting that concept towards the reduction of principles, so that it really is hardly emergence at all.

In fact, it is from this very reductivism that Darwinism has, in large part, derived its convincing power. For what it provided, and provides, is an explanation of evolution in terms of the mechanistic tradition of explanation. A mechanistic explanation must be both *logically simple* and *automatic*. Darwinism satisfies both these demands. First, it is simple, in that it unites all the relevant phenomena under *one* hypothesis and this hypothesis is conceived in simple terms of existence and non-existence. Secondly, it is automatic, since its one hypothesis interprets organisms mechanistically, and their evolution as produced, on the one hand, by random errors (which are the sort of impersonal deviation that can happen to machines despite human efforts to prevent them), and, on the other, by the blind and automatic control of natural selection, an automatism that goes on happening by nobody's intent. It seems likely that the machine model in classical physics derived much of its authority from these qualities of logical simplicity and automatism. Certainly this has been so outside physics. Again and again, faith in our ability to build, in other fields, theories having this double character has motivated new schools of thought in psychology, social science, and philosophy. And it is this faith that supports neo-Darwinism, permitting it to carry, without stumbling, the vast epicyclical superstructure which (as Professor 'Espinasse has pointed out)[1] has been erected upon it. It is this faith also which carries the Darwinian conception past difficulties like those put forward by Schindewolf, without conceiving of them as difficulties at all. In fact, it cannot so conceive them. For to take them seriously as difficulties would be to endanger precisely the logical simplicity and automatism of the neo-Darwinian framework, the very qualities which make it, to its adherents, so *scientific* an explanation. Thus, for example, Schindewolf's view of the sudden origin of basically new types or patterns of organisation implies that the recognition of order, and of novel order, is distinct from the statistical manipulation of the conditions producing order. This, however, is to introduce a duality of logical levels: continuous and small-scale conditions, *versus* discrete and comprehensive pattern; and that means to destroy the *unitary* character of the explanation. Again, to admit orthogenesis, or spontaneous direction in evolution, would be to deny the constant co-variance of gene pool and environment, and thus to suggest as a third factor an inner dynamic in organisms, as distinct both from the non-directive control of random variation and the external steering ' mechanism ' of natural selection. But such a suggestion would deviate from the belief in the *automatism* of evolution. And again, it is precisely the double automatism of gene fluctuation and natural selection that *makes* the neo-Darwinian explanation a scientific explanation in the mechanistic sense. To say, therefore, that the ' facts ' suggest spontaneity in the origin and development of organic pattern is, for convinced Darwinians, not to offer scientific evidence at all, but to step outside the bounds of science. From the Darwinian point of view such ' objections ', which can be formulated only in non-mechanistic language, lie for that very reason beyond the scope of science altogether, and cannot therefore be taken seriously as scientific objections. Thus Sir Ronald Fisher can say in good faith that the only rational theories of evolution are those which make its driving force consist in progressive adaptation. These are the only theories, he says, ' which make at least the most familiar facts intelligible to the reason '.[2] They are indeed the only theories which rest on the ultimate values of total logical simplicity and automatism: within the mechanistic tradition, that is to say, the only rational theories.

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[1] See Polanyi, op cit., Part IV

[2] On the implications of Simpson's use of ' organisation ', see A. Dalcq, ' Le Problème de l'évolution est-il près d'être résolu ? ', *Ann. Ste. Zool. Belg.*, 1951, **82,** 118-138, p. 125

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[1] Paul G. 'Espinasse, ' On the Logical Geography of Neo-Mendelism ', *Mind* (N.S.), 1956, **65**, 75-77

[2] Sir Ronald Fisher, ' Measurement of Selective Intensity ', *Proc. Roy. Soc. B*, 1936, **121**, 58-62

The question remains, however: are the criteria of mechanism (logical simplicity and automatism) adequate as tests of a theory professing to interpret living things, much less the evolution of a living thing ? Simpson, like many mechanistically minded biologists, boasts of following Ockam's razor,[1] which forbids us to multiply entities beyond necessity. As they apply this principle, however, biologists appear to take it to mean that entities must not be multiplied *at all.* There must be only *one* logical level, of physico-chemical events automatically directed this way or that by some equally material relationship. Yet a one-level logic of explanation appears to be unobtainable even in physics and chemistry. In a review of von Laue's *Theorie der Supraleitung,* for example, Professor H. Fröhlich writes:

> ' . . . even an attempt to derive the properties of ideal gases would find the atomic physicist (if assumed to have no knowledge of macro-physics) at a loss without the introduction of new physical concepts. He would probably start with a discussion of the motion of two and then of three weakly interacting particles, and afterwards be led to the conclusion that consideration of more particles is very complicated and unlikely to lead to any simple results. It is only after the introduction of new physical concepts which do not exist in atomic physics—such as pressure and entropy—that other simple laws of physics (the gas laws) can be found.' [2]

If this situation prevails in physics, so much the more so does it obtain in biology. Professor Rosenfeld has recently suggested that biologists should admit, in the use of concepts like ' order ' (of which Schindewolf's ' Bauplan ' or ' type ' is an example), their reliance on a logic of *complementarity:* complementarity holding between concepts of biological organisation and explanations of the physico-chemical conditions of their existence.[3] In the evolutionary context, this general relation becomes the logical duality of new comprehensive ordering principles and the antecedent particulars which were the conditions of their emergence. Such duality is, I believe, unavoidable in evolutionary theory, and should be frankly acknowledged as such.

When, on the contrary, it is not acknowledged, the result is a reductivism which, however brilliant, is basically inconsistent. Some concept of biological organisation, which implies a non-unitary logic, is indispensable to biological theory. Where, therefore, it is ostensibly dispensed with, it nevertheless creeps in again, either in the unformulated presuppositions of the biologist, or in some ingeniously contrived disguise. This kind of procedure: the pretence of understanding a subject matter in terms of fewer and narrower concepts than are in fact indispensable to its understanding, Professor Polanyi has called the fallacy of *pseudo-substitution*.[1] It can be found, I think, at a great many places in the neo-Darwinian literature, and at a number of crucial points in the argument of *Major Features*. I want here to point out one such instance, which is especially instructive in comparison with Schindewolf, that is, Simpson's procedure with respect to the concept of types. ' Type ' or ' Bauplan ' is, as we have seen, a fundamental conception for Schindewolf. In terms of axiomatics, it is a primitive, undefined idea, on the initial understanding of which his theory rests. For Simpson, it is a term to be avoided. In the *Meaning of Evolution* (1949), where he uses the word freely, he explains that this is popularising shorthand, and when he says ' type ' he is not in fact referring to types at all.[2] In *Major Features* he is more careful; his use of the term, or of its equivalent, is, it seems to me, carefully devised so as not to *seem* to mean type although in fact it does so. He gives us first a panoramic view of a continuum of living forms varying continuously in adaptation to a continuously changing environment. Out of this doubly flowing stream, he then distinguishes with some, but not entire arbitrariness, various ' types of adaptation ' or ' adaptive types '.[3] This is, so far, not a phylogenetic concept, nor a morphological one, and if applied strictly could not produce the results Simpson wants from it. Thus, considered purely in terms of *adaptive* types whales would be fish. To prevent this, there must be some structural, some ' type ' reference, implied along with the adaptive one. However, with the continuous stream of life-environment relations as our fundamental concept, we can think of adaptive types as *zones* (the two terms are from here on used as equivalent) cutting themselves out at ' ecological thresholds ' in this continuum,[4] and forget, for the moment at least, the implied structural aspect of the original conception. Since these ' zones ' crystallise out of neighbouring areas in the biological continuum they will in fact be close relatives, so that the whale/fish problem will not arise imaginatively even though in strict

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[1] *Meaning of Evolution,* p. 139

[2] H. Fröhlich, *Nature,* 1948, **161,** 37

[3] L. Rosenfeld, ' Causalité statistique et ordre en physique et biologie ', *Anais da Academia Brasiliera de Ciências,* 1954, **26,** 47-50

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[1] M. Polanyi, op. cit., pp. 169-170

[2] *Meaning of Evolution*, p. 7

[3] *Major Features of Evolution*, p. 200

[4] Ibid., pp. 201 et seq.

logic it should have arisen. Such ‘ zones ’ are, moreover, Simpson explains, only figuratively ‘ zones ’; they represent, he says, ‘ a characteristic reaction and mutual relationship between environment and organism, a way of life and not a place where life is led ’.[1] But calling them zones, we can think of them almost as places, as being ‘ entered ’ and ‘ occupied ’. Thus the need to separate out the organism as an entity, and *a fortiori* the segmentation, the ‘ type ’ aspect, is reduced and concealed.

Yet ‘ adaptive zone ’ is introduced as a *synonym* of ‘ adaptive type ’. Although the ‘ type ’ aspect is blurred, it is nevertheless taken for granted, for the theory would not work without it. Adaptation pure and simple is an insufficient instrument for the separation of different kinds of animals or plants: as, once more, in the case of whales and fish. In practice, of course, Simpson does recognise types—general patterns of structure or function—all along. Even to count the number of enamel plates per 10 mm. of an elephant's tooth means recognising some kind of organisation of jawbones and teeth. To notice the threshold at which the artiodactyl and perissodactyl limb separated out demands knowing horses and elephants from antelopes and cows. Moreover, the concept of ‘ adaptive zone ’, by providing a more elastic substitute for ‘ type ’, allows us to forget the existence of such comprehensive knowledge.

10 *Conclusion*

We should conclude, I think, that where concepts of more than one logical level are necessary to the interpretation of a set of phenomena, we ought not to pretend to be operating on one level only. In the context of evolution where we in fact acknowledge novel operational principles, we should not pretend that nothing is there but the conditions without which (admittedly) they could not operate. The sum total of necessary conditions for the coming into being of an individual, a species, a phylum or of life itself are not logically or historically identical *with* the individual, or species, or phylum, or life itself. The pretence that the whole story can be told in terms of conditions alone, that flux can generate form, that materials not yet organised can logically account for their own organisation, is a pseudo-substitution, which purports to do without what it cannot in fact eliminate.

This situation is, I believe, unavoidable in a reductive theory of evolution, a theory which attempts to treat mechanistically a subject matter radically incapable of such treatment. I suggest, therefore, that instead of Ockam's razor we might adopt as a test of theories of evolution the opposite principle: that entities, or more generally perhaps aspects of reality—for principles of organisation are not entities, though they do define entities—should not be *subtracted* beyond what is honest. In the light of this principle, we should ask of any theory of evolution, does it pretend to do without concepts which in fact it does not do without ?

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[1] *Major Features of Evolution*, pp. 201–220

Here Schindewolf's theory, I think we must conclude, is more adequate than Simpson's. The concept of type, or of organisation in some form, is indispensable, and if banished makes its way in again, or has been there, surreptitiously, all along. Secondly, the concept of novelty or originality, which again Schindewolf admits as a fundamental concept, is indispensable. Simpson's theory, being more complex and abstract, would appear at first sight to cover a wider range of phenomena; yet by the reductive nature of its abstractions it also overlooks essential aspects of the phenomena. Schindewolf's theory is simpler in its reasoning, and less unified in its conceptual structure, but, perhaps for that very reason, it remains closer to the phenomena and does more justice to their experienced complexity.

Finally let me suggest briefly, the bearing of this particular study, limited though it necessarily is, on the epistemological implications of the problem of evolution. The problem of evolution is to explain how new forms of life have originated. But new forms of life embody new operational principles, and these must be recognised as such before we can so much as ask about the continuity of the conditions out of which they arose. True, they could not have arisen without those conditions, but neither are they, logically or ontologically, identical with them. This means, I suggest, that we should apply to evolutionary theory an historical form of Kant's transcendental method. As latecomers in evolution, we should ask: what are the necessary presuppositions of the history we believe we are ? What are the minimal concepts through which we can assess that history as having happened, and without which we could not understand that it did happen ? If, as philosophers reflecting on evolution, we wish to ask this question, we shall find in Schindewolf's theory at any rate nothing incompatible with asking it, whereas if we commit ourselves to neo-Darwinism, we shall be unable to understand, much less to undertake, such an enquiry.

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CAUSAL EXPLANATIONS IN NATURAL HISTORY * †

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THERE are certain empirical sciences such as geology, palaeontology, and biology whose subject-matter is partly historical. They investigate phenomena which originated and existed for varying periods of time in the remote past, long before the appearance of men on the earth. One aim of these sciences is to describe as fully as possible each of the phenomena in question by utilising available evidence. Often, however, this evidence is fragmentary. Hence a particular phenomenon may have to be reconstructed by supplying conjectured details to fill in the gaps. But the sciences concerned with natural history do not always stop here. In addition to describing, they also formulate causal explanations of the phenomena with which they deal.

Some philosophers of science seem to me to have done less than justice to this subject. Thus Toulmin takes the position that there are no causal explanations at all in natural history, and little that deserves to be called ' explanation ' in any sense. The sciences concerned, he thinks, are restricted to making descriptive generalisations based on the observation of elementary regularities.[1] Braithwaite, on the other hand, agrees that biologists and others sometimes ask historical questions about the origin of particular events, and seek to give causal answers to those questions. But he holds that ' to ask for the cause of an event is always to ask for a general law which applies to the particular event '.[2] In the present paper I want to suggest some reasons for disagreeing with both of these positions by showing (*a*) that causal explanations do occur in natural history, and (*b*) that some, at least, have logical features which differentiate them quite sharply from causal explanations which apply a general law or set of laws to particular events.

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* Read at a meeting of the Eastern Division of the American Philosophical Association, 28 December 1957.

† Received 21. i. 58

[1] S. E. Toulmin, *The Philosophy of Science*, London, 1953, pp. 44 sqq.

[2] R. B. Braithwaite, *Scientific Explanation*, Cambridge, 1953, p. 2

CAUSAL EXPLANATIONS IN NATURAL HISTORY

I

Sciences concerned with natural history seek causal explanations for two main types of phenomena. The first type consists of the origin or genesis of single objects, particular kinds of objects, and particular states of affairs. Thus explanations are sought for the origin of the moon, the origin of reptiles, the first occupation of dry land by the vertebrates, and so on. The second type of phenomena consists of the disappearance of single objects, or particular kinds of objects, and the termination of particular states of affairs. Thus students of natural history try to explain why the dinosaurs or the Irish elk became extinct. I will restrict my remarks to these two types of phenomena, and will focus attention only on specimens of explanation found in the biological sciences.

When a biologist investigates a phenomenon of natural history, what he frequently wants to explain is an individual occurrence, something which has 'come to be' or 'ceased to be' *just once*. The phenomenon is therefore non-recurrent, and hence not subject to controlled experimentation. The precise circumstances under which it arose or disappeared, including the temporal interval involved, cannot be duplicated. Its spatio-temporal boundaries are impossible to specify exactly. Nevertheless, the aim of a biologist may very well be to account for the genesis or the termination of *each* such individual phenomenon. To accomplish this aim he may try to reconstruct the causal conditions which were operative, and to arrange them in an explanatory pattern.

Now it is generally agreed that from a strictly logical standpoint the causal explanation of any phenomenon, P, ought to conform to the requirements of one or the other of two schemata. Either (*a*) the explanation ought to indicate those conditions temporally prior to (or simultaneous with) P which are independently necessary and jointly sufficient for the occurrence of P; or (*b*) the explanation ought to indicate those conditions temporally prior to (or simultaneous with) P which are jointly sufficient but not independently necessary for the occurrence of P. The second case obviously allows for the possibility that there may be a disjunctive set of sufficient conditions of P ('plurality of causes'). Yet each member of this set has to include all the necessary conditions of P together with enough contingent contributory conditions to permit a sufficient condition of P to be constituted. In actual practice, both of these schemata represent ideals, rarely, if ever, attained even by the so-called ' advanced sciences '. The causal explanations with which I am concerned are at best approximations to schema (*b*). That is to say, they are explanations which seek to formulate a possible sufficient condition of P by specifying as many of the necessary and contingent contributory conditions of P as can be discerned or imagined. Sometimes the sufficient condition is first entertained in a vague form, and then its vagueness is gradually reduced by ' filling in ' a number of the necessary and contributory conditions which it embraces. This also increases its testability, as I will try to show. But the strict requirements of schema (*b*), let alone (*a*), are never met. Hence a large degree of flexibility attaches to the notion of causal explanation in natural history.[1] The particular pattern which I propose to consider here is, therefore, not put forward as the *only* one which natural history exhibits. I have no wish to construct a Procrustean bed on which all explanations have to be fitted, especially in a paper which is to a large degree exploratory.

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Let us now look at an actual specimen of causal explanation in natural history. It is advanced to account for a particular occurrence, namely, the occupation of dry land by the first vertebrates, the primitive amphibians which inhabited fresh-water pools and streams in Carboniferous and Devonian times, and which possessed rudimentary limbs. I will display the specimen at some length before commenting on it.

> ' Why should the amphibians have developed these limbs and become potential land-dwellers? Not to breathe air, for that could be done by merely coming to the surface of the pool. Not because they were driven out in search of food, for they were fish-eating types for which there was little food to be had on land. Not to escape enemies, for they were among the largest animals of the streams and pools of that day.
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> The development of limbs and the consequent ability to live on land seem, paradoxically, to have been adaptations for remaining in the water, and true land life seems to have been, so to speak, only the result of a happy accident. . . .
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> The Devonian, the period in which the amphibians originated, was a time of seasonal droughts. At times the streams would cease to

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[1] Cf. F. A. Hayek, 'Degrees of Explanation,' this *Journal*, 1955, **6**, 209-225. I am indebted to certain of the ideas expressed in this paper.

> flow. . . . If the water dried up altogether and did not soon return, . . . the amphibian, with his newly-developed land limbs, could crawl out of the shrunken pool, walk up or down the stream bed or overland and reach another pool where he might take up his aquatic existence again. Land limbs were developed to reach the water, not to leave it.
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> Once this development of limbs had taken place, however, it is not hard to imagine how true land life eventually resulted. Instead of immediately taking to the water again, the amphibian might learn to linger about the drying pools and devour stranded fish. Insects were already present and would afford the beginnings of a diet for a land form. Later, plants were taken up as a source of food supply. . . . Finally, through these various developments, a land fauna would have been established.' [1]

This piece of reasoning has a number of distinctive features. Although it is part of a scientific discussion, it is not concerned to discover or to confirm a general law. It does not establish any new empirical fact which is simply to be added to the store of human knowledge. It does not make an explicit, positive prediction about what will be found by future investigations, though there is a sense in which it makes certain negative predictions, as I will suggest below. What the reasoning does is to propose a theoretical pattern composed of accepted statements of facts (' The Devonian . . . was a time of seasonal droughts '), statements inferred from these statements of facts (' At times the streams would cease to flow . . .'), statements which are plausible conjectures about various occurrences (' The amphibian . . . could crawl out of the shrunken pool, walk up or down the stream bed . . . learn to linger about the drying pools and devour stranded fish . . .'), etc. This pattern presents an intelligible sequence of events such that the phenomenon to be accounted for ' falls into place ' as the terminal phase of the sequence. The phenomenon ceases to be isolated, and is connected in an orderly way with states of affairs which led up to it.

Looking more closely at the reasoning, we can say that what it does is to formulate by means of the theoretical pattern a possible sufficient condition of the phenomenon to be explained.[2] This condition is both complex and formulated in broad terms against a background of assumptions, theories, and information which serves to limit it. The explanatory pattern has specifiable ' boundaries ' which allow it to be presented as a relatively isolated unit. Thus, although the explanation of the origin of land fauna presupposes the origin of vertebrates, we do not have to explain the latter phenomenon in order to explain the former. The pattern specifying the possible sufficient condition involves a number of components which the reasoning undertakes to enumerate. These components are either statements of necessary conditions (e.g. that the amphibians must have emerged from the water to become land dwellers, that they must have found suitable food after they emerged, etc.), or statements of contingent contributory conditions (e.g. that the amphibians walked up or down stream or overland, etc.). It is fairly evident that no exhaustive list of the component conditions is given. It is also evident that where conjectured events are introduced into the pattern, their plausibility depends on the relevance they have to the over-all sufficient condition. Without the latter there would be no basis for deciding what events can be plausibly included in the pattern and what events must be excluded. This is the main, though not the only, controlling factor in the situation.

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[1] A. S. Romer, *Man and the Vertebrates*, 3rd edn., Chicago, 1941, pp. 47-48

[2] To say this is, of course, to give an analysis or reconstruction of the reasoning in the light of a logical schema. It might be argued that what the example cited really offers is a ' teleological ' not a ' causal ' explanation. Yet if one takes the explicandum to be contained in the question, 'Why did the primitive amphibians become land dwellers? ' the explanation makes no reference to purposes, ends, or

It is tempting to think of such a causal explanation as conforming to the model of a deductive system. One is inclined to feel that the statements making up the explanation must be capable, at least in principle, of being organised deductively or perhaps even axiomatically. But this seems to me to be the wrong model to have in mind. As the above example shows, an explanatory pattern in natural history is closer to the model of a *coherent narrative* than to that of a deductive system. The narrative ' pictures ' a certain temporal sequence of events. But the sequence is not such as to allow each event to be deductively inferred from its predecessors. For the events specified do not constitute just the necessary conditions of the phenomenon, and certainly do not constitute, in most cases, all those conditions. As a rule, only the necessary conditions which are ' crucial ' or ' most

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goals. No biologist is likely to suggest that the activities of the amphibians were ' directed ' towards the establishment of land fauna. This result was only ' a happy accident '. Even if we include as part of the explicandum the question, ' Why did the amphibians develop land limbs? ' we cannot give the ' teleological ' answer, ' In order to be able to live on the land,' but have to say, according to the example, ' In order to remain in the water '. A causal reconstruction of the reasoning seems, therefore, to be what is required.

important' are stated. Along with them are included a variety of contingent contributory conditions. Hence, given the determinate temporal order of the events pictured by the explanatory pattern, the logical relations among many of the component statements are merely conjunctive, not implicative. I do not deny, of course, that implicative relations do obtain between *some* statements of the pattern. But this is clearly not enough to yield a deductive system.[1]

The suggestion may be advanced that what we are concerned with here is really a causal chain of events. The temporally successive stages represented by the pattern make up a linear series, each member of which is the effect of its immediate predecessor and the cause of its immediate successor. The first 'link' of the chain is an event whose predecessor we disregard; and the final 'link', being the event we want to explain, is one whose successor we disregard. This analogy of a causal chain is, however, quite unsatisfactory, as N. R. Hanson has recently shown.[2] In the present case the analogy tends to obscure the fact that each component of the pattern is specified so as to be compatible with the existing body of scientific knowledge, and would be unintelligible apart from that knowledge. Furthermore, the analogy falsely suggests that the specification of the components is exhaustive.

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[1] Hence the model of a hierarchical deductive system, so often presented as the ideal to which the theoretical part of *every* science should approximate, is not relevant to the sciences concerned with natural history, however much it is relevant to non-historical sciences.

It may be objected that the proposed reconstruction of the reasoning does not escape being deductive. For if, as suggested, the individual phenomenon, P, is explained by specifying a complex sufficient condition, S, then we are entitled to affirm the generalisation, 'Whenever S, then P.' But if so, the reasoning is clearly an ordinary mixed hypothetical syllogism: 'Whenever S, then P. But S. Therefore, P'.

To this objection several replies can be given. (i) In actual practice, S is specified as a *possible* sufficient condition of P, and hence we are *not* entitled to affirm the generalisation, 'Whenever S, then P'. (ii) Even if, after prolonged investigation, we become convinced that S was in fact the sufficient condition of P on the particular occasion in question, we should not be entitled to affirm 'Whenever S, then P' *as an empirical generalisation*. For we have just the one, non-recurrent case, P, to go on; and no biologist would wish to generalise from a single instance. It is true, that the assertion of S as a sufficient condition of P entitles us to affirm *the logical principle*, 'If S, then P'. Furthermore, this is a principle accepted by the biologist when he argues, 'P because S'. But it is not a principle from which the latter assertion follows deductively. Cf. G. Ryle, '"If", "So", and "Because",' in *Philosophical Analysis: A Collection of Essays*, Edited by Max Black. Ithaca, 1950, pp. 323-340

[2] N. R. Hanson, 'Causal Chains', *Mind*, 1955, 64, 289-311

(One cannot have a ' chain ' if links are missing.) In short, the causal chain image tempts us to overlook the importance of contextual considerations here, as elsewhere. Each explanatory pattern of the sort we are considering is less like a segment of an isolated causal line stretching back indefinitely into the past, and more like a portion of a complex historical network with an enormous number of cross-connections.

The choice of the over-all pattern as a possible sufficient condition of the phenomenon is made in the light of existing knowledge or reasonable inference therefrom. In the example quoted, the initial move is to eliminate certain alternative possibilities because they are incompatible with accepted facts. (The amphibians did *not* become land-dwellers because they were driven out of the water by the need to breathe air, or to obtain food, or to escape from predators.) Another alternative (that the ability to live on land was an adaptation for continuing to live in water) is compatible with everything known. But it is formulated in such a way as to be both vague and paradoxical. To remove its paradoxical character and reduce its vagueness, a number of the component events are narrated in temporal sequence. Obviously, the selection of these events depends not only on their relevance to the over-all pattern and their compatibility with the general body of scientific knowledge, but also on the demands of internal coherence imposed by the sequential character of the total narrative. The narrative has to tell a ' likely ' story. It is worth noting how much use is made throughout not of the simple past tense, but of the possibility-expressing auxiliaries, ' could ', ' would ', and ' might '. They show the conjectural nature of many of the constituents of the pattern.

Another side of this matter will be highlighted if we take note of a case where natural history has *failed* to find a causal explanation of a phenomenon. It is well known that no-one has been able to offer an acceptable explanation of the extinction of the dinosaurs. The difficulty here seems to be that there are *too many* possible factors available, rather than too few. Palaeontologists are unable to make a selection from the numerous alternative possibilities, and thus construct a plausible explanatory pattern. As an eminent palaeontologist puts it:

> ' No one knows exactly why dinosaurs became extinct. This is not because there is anything mysterious or metaphysical about extinction or because possible causes are unknown. It is just because there are many reasonable possibilities, and the record does not enable us to say in the particular case which of them were actually involved.

> All we can say at present is that something changed and the dinosaurs did not.'[1]

Note that it would be considered unsatisfactory to suggest as an explanation some very general sufficient condition of the phenomenon, e.g. that dinosaurs became extinct because they failed to adapt to environmental changes. This suggestion fails to ' fit ' the particular phenomenon tightly enough. To make the proposed explanation satisfactory in a minimal sense, some of its components (necessary or contributory) would have to be reconstructed in temporal sequence. Where, as in the present case, it is impossible to choose a *specific* sequence of components from the wide range of possibilities available, an acceptable explanation cannot be put forward.

3

At this point it may be objected that if the above account is correct, then the causal explanations in natural history which have been discussed all suffer from a fatal defect. They are insusceptible to any sort of empirical testing. We can, indeed, ensure that a given theoretical pattern is internally coherent and compatible with existing knowledge. But since it involves no general laws and makes no positive predictions, we can never subject it to any verificational procedure. It remains a sheer conjecture about a past event.

To this objection the reply may be made that in as much as an explanatory pattern specifies a determinate sequence of events (or conditions), it restricts the possibilities with regard to what future empirical investigations will disclose. The pattern does, therefore, involve a number of negative predictions. It implies *that certain phenomena will not turn up in any future inquiries* (e.g. in palaeontology, comparative anatomy, experimental biology, etc.). If one of these phenomena does turn up, it will invalidate or falsify the pattern. Hence, such causal explanations in natural history *are* testable, for nothing prevents their inadequacy from being detected, in the long run, if they are inadequate. Since Popper has given powerful reasons for believing that a distinguishing mark of a scientific theory is its falsifiability, i.e. the possibility of overthrowing it on the basis of empirical evidence, these causal explanations can be regarded as intrinsically no less scientific than (though in other respects quite different from) explanatory theories in the non-historical sciences.

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[1] G. G. Simpson, *Life of the Past: An Introduction to Palaeontology*, New York, 1953, p. 139

This consideration provides a rule of selection in situations where more than one explanatory pattern relevant to a phenomenon, P, can be formulated. For suppose that E_1 and E_2 are a pair of such patterns; and that in the case of E_2 we are able to specify a smaller number of the component conditions (necessary and contributory) than in the case of E_1. It follows that E_1 will then be more readily shown to be indequate than E_2. In other words, E_1 is a more testable pattern than E_2, because the temporal sequence of conditions presented in the case of the former involves a larger number of negative predictions than does the temporal sequence presented by the latter. This fact should clearly lead us to choose E_1 rather than E_2 as a causal explanation of P. But I think Popper is right in affirming that no explanatory theory can ever be conclusively confirmed. It may be progressively confirmed, of course, as the result of a long series of unsuccessful attempts to overthrow it. At bottom each explanatory pattern in natural history is an ' informed conjecture ' about the origin or the termination of some past phenomenon—not a categorical affirmation of the truth, but an attempt to find out the truth about it.[1]

I submit, then, that natural history does employ causal explanations, and that the most typical, being the sort I have attempted to describe, do not bring an individual phenomenon under a general law. Furthermore, these typical explanations seem to me wholly appropriate to the subject-matter with which natural history is concerned. The method used in constructing them is entirely sound. There is no superior method (e.g. that of physics) which ought to be substituted for it. Hence, no warrant exists for saying that all explanations found in the historical sciences are ' weak ', or for suggesting that these sciences are still at a rudimentary stage of development. Physics is not necessarily the adult form of every science, and natural history is not adolescent physics.[2]

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[1] The *locus classicus* of Popper's discussion is, of course, his *Logik der Forschung*, Vienna, 1935, now happily promised in an English translation, *The Logic of Scientific Discovery*, London. Cf. also his two stimulating essays, ' Three Views Concerning Human Knowledge ', in *Contemporary British Philosophy*, 3rd Ser., edited by H. D. Lewis, London, 1956; and ' Philosophy of Science: A Personal Report ', in *British Philosophy in the Mid-Century*, edited by C. A. Mace, London, 1957.

[2] For other aspects of the subject of explanation in the historical sciences, see especially: W. B. Gallie, ' Explanations in History and the Genetic Sciences ', *Mind*, 1955, **64**, pp. 160–180; ' In Reply to Mr Montefiore ', *Mind*, 1958, **67**, pp. 92–96; and R. P. Gould, ' The Place of Historical Statements in Biology ', this *Journal*, 1957, **8**, 192–210.

R. B. BRAITHWAITE ON PROBABILITY AND INDUCTION *[1]

Henry E. Kyburg, Jr.

R. B. Braithwaite devotes nearly half of his recent and fascinating book, *Scientific Explanation*,[2] to a consideration of the related problems of probability and induction. I do not find what he has to say about induction completely satisfactory, and I should like to give my reasons for this feeling and for tracing it back to his treatment of probability and statistical inference.

I

Braithwaite begins his consideration of the justification of induction with an excellent statement of the problem: ' We do not come to the discussion of how we obtain reasonable belief in a scientific hypothesis already knowing what we mean by such a " reasonable " belief and . . . such a " valid " inference; in stating the conditions which justify inferences we shall, *ipso facto* be giving criteria which determine the meaning of the phrases " valid inference " and " reasonable belief " in the case of an inductively established hypothesis.'[3] This is precisely the problem: to elucidate the meanings of these honorific epithets.[4]

Braithwaite approaches his solution by proposing that we stand back and look at what all inductive inferences have in common. ' What then becomes clear is that all the inductions whose validity we wish to establish are inferences in which the inductive conclusion has been derived from the empirical premises in accordance with one or other of a limited number of inductive principles of inference.'[5] He

* Received 27. ii. 58

[1] This article is adapted from Chapter Five of my dissertation, *Probability and Induction in the Cambridge School* (University Microfilms, Ann Arbor, 1955), which was prepared at Columbia University under the direction of Professor Ernest Nagel, to whom I am indebted for much excellent advice.

[2] R. B. Braithwaite, *Scientific Explanation*, Cambridge, 1953

[3] Ibid. p. 257

[4] Cf. my article, ' The Justification of Induction ', *Journal of Philosophy*, **59**, 1956

[5] Braithwaite, op. cit. p. 260

does not want to go so far as to say that this is all there is to the solution of the problem, for it is obviously possible to call the principles of inductive inference themselves into question. He therefore formulates what he calls the *predictionist* justification of the principles of inductive inference, for which he acknowledges indebtedness to Peirce. The justification for an inductive policy (principle of inference) is that by following it we are led to hypotheses which are generally confirmed, and not refuted, by subsequent experience.

This is formulated in more precise language, and given expression in the following definition of an *effective inductive policy:*

> *Π* is an effective inductive policy if and only if,
>
> 'Of every time *t* later than a fixed time t_0, and of every interval of time of a fixed length of years *d* lying within the interval (t_0, *t*), it is true that many of the hypotheses established by the use of policy *Π* during the interval of *d* years (unless there are no such hypotheses) have the joint property (1) of not having been empirically refuted at any time between the time of establishment and *t*, and (2) of having been empirically confirmed at least once between the time of establishment and *t*.'[1]

An inductive policy which is *effective-in-the-past* is one which satisfies the above criterion, except that 'of every time *t* not later than the present time' is substituted for 'of every time *t*'.

Having presented these definitions, he goes on to say, 'It is a historic fact that the inductive policies of good scientific repute are effective-in-the-past policies; it is a general hypothesis that they are effective as well as being effective in the past'.[2] It clearly follows that '*the effectiveness of an inductive policy is an empirical proposition* which does not logically follow from the policy's effectiveness-in-the-past'.[3] But the effectiveness of the inductive policy concerned is the ground on which the epithets 'valid' and 'reasonable' are applied to an inductive argument and its conclusion, respectively. 'The thesis maintained in this chapter is that the effectiveness of the inductive policy concerned is a sufficient condition for the adjective "valid" to be applied to an inductive inference from known evidence and for the adjective "reasonable" to be applied to belief in the conclusion of the inference.'[4] Braithwaite apologises for the weakness [*sic*] of this criterion of validity in the words: 'But if all that can be said in favor of using an inductive policy is that it frequently predicts successfully, is this a justification for basing actions upon beliefs obtained by such a policy? '[1] After a struggle with his conscience, he decides that it is; but the implication remains that we can and ought to have a guarantee that a given inductive policy will ' frequently predict successfully '.

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[1] Braithwaite, op. cit. p. 267

[2] Ibid. p. 268

[3] Ibid. p. 269. My italics.

[4] Ibid. p. 270-1

There is little reason to dispute the sufficiency of the criterion in question—partly because of the indefiniteness of the interval of time *d*, the location of the given time t_0, and the number represented by ' many '. We *could* dispute it: A successful swami may very often predict successfully by means of his mysterious method of crystal-gazing, deep breathing, and (what we would call) free association. But the fact that many of his more indefinite predictions are vindicated does not give us any great warrant for expecting his more specific predictions to be vindicated. Braithwaite does not mention the possibility that while many hypotheses established by a given method are confirmed and not refuted by future experience, many, many more, established by the same method, are refuted and not confirmed by future experience. Would this not constitute grounds for preferring some other method?

Braithwaite implies, though he does not explicitly claim, that his criterion is necessary as well as sufficient. While the insufficiency of the criterion can be taken care of by adding a few clauses to it, the question of whether or not it can be plausibly maintained that the criterion is a necessary condition for the validity of an empirical inference, raises even more interesting problems. If we are to justify induction at all, and if the criterion cannot be satisfied, then it is clearly unplausibly stringent. Braithwaite claims that the criterion can be satisfied; he says that we can know that a given inductive policy is effective because it can, in some cases, be established as effective by another inductive policy. ' That policy Π is effective is an inductive hypothesis to be established by induction according to a principle of simple enumeration on the basis of its effectiveness-in-the-past.'[2]

2

We can justify belief in the hypothesis that Π is an effective policy, given (*a*) that Π is an effective-in-the-past policy, and (*b*) that the policy of induction by simple enumeration is an effective policy. So far, if we charitably ignore the details of this justification, so good. But immediately the question arises as to whether it is possible to justify

[1] Braithwaite, op. cit., p. 272

[2] Ibid., p. 273,

the inductive policy of induction by simple enumeration in this same way. Braithwaite answers this question in the affirmative; I shall go into his argument in some detail, since if it is valid, it is extremely important: any number of inductive policies may turn out to be self-justifying.

The argument is as follows: 'Let Π be the inductive policy of adding belief in a hypothesis h to belief in a set of propositions which collectively constitute the π-evidence for h, . . . π being the principle of induction by simple enumeration. Let e be the proposition that the policy Π is effective.'[1] 'The truth of e justifies the use of policy Π; i.e. for every h the truth of e justifies an addition of belief in h to reasonable belief in π-evidence for h.'[2] Substituting e for h, we get: the truth of e justifies an addition of belief in e to reasonable belief in π-evidence for e. It is then claimed that this argument escapes from the *petitio principii* sort of circularity by virtue of the fact that e must merely be true—it need not, in addition, be reasonably believed, or, for that matter, believed at all. We may still ask, however, whether or not the inductive inference in question is satisfactory.

This leads Braithwaite to the general question of what it means to call an inference valid.[3] He considers the case of a person B inferring q from p by means of a principle of inference whose effectiveness is asserted by r. He lists ten possible circumstances, but only the following three are relevant :

VI. B reasonably believes p and believes r.
VIII. B reasonably believes p and r is true.
IX. B reasonably believes p and believes r and r is true.

An inference which proceeds in accordance with VI is called subjectively valid; one which proceeds in accordance with VIII is called objectively valid; and one which proceeds in accordance with IX is both subjectively and objectively valid.

Braithwaite claims that in any of these three senses, the inference to e by way of Π and π-evidence for e is valid. 'To justify the use of the policy of induction by simple enumeration by its effectiveness, as we have done, is to use criterion VIII for the objective validity of the inference to the hypothesis of its effectiveness.'[4] Since Braithwaite has expressly denied that the effectiveness of Π is known *a priori*, this argument boils down to: *If* Π is effective, *then* we can *reasonably* infer the effectiveness of Π from the π-evidence for the effectiveness of Π. In other words, since we have abundant evidence (presumably) for the effectiveness of Π, we may say that *if* Π is effective, *then* we may reasonably believe that Π is effective. Although I am willing to agree that this argument avoids circularity, it is nevertheless clear that it does not answer to Braithwaite's need for a *justification* of the policy of induction by simple enumeration. We must be able to assert the antecedent, ' Π is effective ' if we are to be free to assert the consequent, ' We may reasonably believe that Π is effective ', by *modus ponens*. Although it may be that in fact the antecedent is true, *we* cannot assert the consequent unconditionally unless we can *assert* the antecedent—which we obviously cannot. Only God, who presumably knows whether or not Π is an effective policy, can know whether our belief in the effectiveness of Π is reasonable or not. All *we* can say unconditionally is that our belief *may* be reasonable.

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[1] Braithwaite, op. cit. p. 276

[2] Ibid. p. 277

[3] 'Cogent', I think, would be a better term. By 'valid' one generally implies only that the inference does in fact conform to the principle of inference to which it is alleged to conform; cf. my article in the *Review of Metaphysics* (September, 1958).

[4] Braithwaite, op. cit. p. 281

For those who find this sense of ' validity ' unsatisfactory, Braithwaite proposes the subjective sense, as exemplified in inferences of type VI. In this form of inference, we proceed from mere belief in the effectiveness of Π to *reasonable* belief in its effectiveness. This form of inference also avoids the *petitio principii* sort of circularity. But while the former argument can be employed only by God—and thus, in the mouths of mortals, does not prove enough—this argument seems to prove too much. It certainly does not entail that the use of policy Π will in point of fact enable us to predict successfully in the future; it states that it is reasonable (in a certain sense of ' reasonable ') to believe this; this sounds plausible enough, but it would seem to be just as plausible to start out by asserting that it is reasonable to believe in the effectiveness of Π, on the grounds that this is part of what we *mean* by reasonableness. For that matter, we may assert that it is reasonable, by definition, to believe in conclusions obtained by the use of inductive policies which are effective-in-the-past, dropping all reference to future effectiveness. As it stands, the argument proves too much because it can be applied to policies which do not, to most of us, seem reasonable at all. This may be illustrated by an example.

Let r assert that the Oblonsky Method of tea-leaf reading is a highly successful method of predicting the future; let q assert that in the future the application of this method will lead to many true conclusions; and let p be the empirical proposition that the tea-leaves in my morning cup of tea lay in such a way that the Oblonsky Method predicts q on the basis of p. If I believe in the Oblonsky Method, then by analogy with Braithwaite's type VI inference, I may *reasonably* believe q. The inference from the empirical premise p to the conclusion that in the future the Oblonsky Method will lead to many true conclusions is subjectively valid. Indeed, if the Oblonsky Method really is a highly successful way of predicting the future, the inference is objectively valid as well. The objection that the method has (in crass scientific terms) been ' refuted ' in the past, is simply irrelevant: I am interested in the future successes of the method, not in its past failures. And I am just as free to employ the Oblonsky Method to refute the hypothetical effectiveness of induction by simple enumeration (at least as applied to the Oblonsky Method) as I am to employ the effectiveness of induction by simple enumeration to refute the hypothesis that the Oblonsky Method will lead to increasingly frequent success in the future. Both methods are in the same logical boat.[1]

Even if we could accept Braithwaite's argument for believing in the effectiveness of the policy of induction by simple enumeration, there would remain difficulties connected with this justification of induction. The criteria for effectiveness-in-the-past are vague, involving reference to ' many ' hypotheses, a span of years d, and so on. Braithwaite does not describe any inductive policies in detail, so that we are not quite sure what constitutes a ' policy '. If we interpret ' policy ' in very broad terms, then we must be prepared to say that some policies are more successful in some areas of application than in others; if we interpret ' policy ' in a narrow sense, then we may find that we have very little enumerative evidence in favour of the effectiveness of most of the prevalent inductive policies; if we interpret ' policy ' in still narrower terms, we will find that every inductive inference that was ever made employed its own unique inductive policy. Thus the policy of inferring from a sample of goldfish collected from pond P on 30 January 1958, that the mean weight of the goldfish in pond P is w grams, has never been used before and will never be applicable again.

Furthermore, even if there is no circularity in an inductive argument purporting to support induction, there does seem to be an aura of redundancy about it. If a given inductive policy is effective, it is so in virtue of some empirical truth. If we isolate this true (or reasonably credible) empirical element, then, employing a statement of this element among the premises, together with the empirical data which provide the grounds for the inference, we will have an inference which can be shown to be demonstrably credible. Thus if we can show that the statement, ' Samples of size N, of objects of type T, selected by method M, are representative of the mean weight of all such objects of type T ' is rationally credible, then surely there is no difficulty about showing that the consequence inferred from this statement in conjunction with appropriate data statements, ' The mean weight of objects of type T is W ', is rationally credible. If we can show that the empirical hypothesis is well confirmed, then we do not have to ascend to inductive *policies* at all. On the other hand, if we cannot show that the hypothesis is well confirmed, then I do not see how we can get away with incorporating it in an inductive policy; to incorporate it in an inductive policy is tantamount to assuming that it is, if not true, at least rationally credible.

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[1] Though even Oblonsky would no doubt hesitate to argue that his method had ' refuted ' the general method of induction by simple enumeration.

Finally, there is the question of practicality. While there are many people who would like to see a general justification of induction, and while the search for such a justification is certainly interesting and worthy, there is a more immediate reason for concern over the problems of induction and of probability. Competent investigators in the sciences usually agree concerning whether or not the acceptance of an empirical generalisation is warranted by the evidence, but they do not *always* agree.[1] The canons of inductive inference in accordance with which they could always come to agreement have not been made explicit. If they were, such disagreements as there are could be avoided, barring only those which arise with respect to theories in which an intuitive sense of the meaning of the theory in relation to the entire field seems to play (if only initially) an important rôle in determining its credibility.

These last three paragraphs cannot be taken as criticism fatal to Braithwaite's point of view; all three objections could be met by a more detailed and explicit characterisation of definite inductive policies —preferably all of that ' limited number ' to which Braithwaite refers. Since it was not his object in *Scientific Explanation* to carry through such a detailed analysis, he can hardly be held culpable for not having done it. But we can reasonably expect his treatment of induction by simple enumeration to be rather detailed, since it is on this policy that all of the other inductive policies must ultimately depend. Since induction by enumeration is a special case of statistical inference, I shall now turn to Braithwaite's theory of probability and his analysis of statistical inference.

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[1] The most dramatic case in point is that of the hypothetical existence of ' psychic ' phenomena. A more pedestrian example is the hypothetical relationship between smoking and cancer.

3

Braithwaite gives an interpretation of probability as it is used in statistical hypotheses which is new and very attractive in its simplicity. He begins by stating that one of the natural uses of probability is for expressing a proportion: 'Instead of saying that the proportion of male births among births is 51 per cent., we may say that the probability, or the chance, of a birth being a male birth is 51/100.'[1] This is the only sense of probability which Braithwaite considers useful in relation to the problems he is considering. 'What a philosopher of science is concerned with is, first, probability as it occurs within the hypotheses of a science, i.e. with the concept of probability as the characteristic of statistical hypotheses; and, secondly, probable in the sense of reasonable as applied to scientific hypotheses themselves or to belief in them. For the rest of this book the word will be used only for the empirical, numerically measurable concept characteristic of statistical generalisations.'[2]

He proposes to interpret probability as it is used in statistical hypotheses by means of a model built along the following lines:

> To the probability statement that the probability of a child born being a boy is 51/100 and to the observed class-ratio statement that among 1000 children born 503 are boys, there is to correspond, respectively, a set-up of the model with a set of 1000 bags each containing 100 balls 51 of which are black, and a draw, one from each of the 1000 bags, of 1000 balls 503 of which are black. To the proposition about the observed class-ratio there corresponds not a draw of balls forming a *subclass* of a class whose proportion of black balls is 51/100, as in the hypothetical infinite population model, but a draw of balls forming a *selection* from a class of classes, the proportion of black balls in each of which is 51/100. The new model allows for the possibility of a draw of increased size by increasing the number of bags while keeping the number of balls in each bag constant.[3]

If we denote the class of balls by β, and the class of black balls by α, we can define the α-parameter of the (α, β)-hyperclass consisting of all the individual bags, as being the class-ratio of black balls to all the balls in each bag. A statistical hypothesis stating the probability than an α will be a β, can be interpreted in a model in which the (α, β)-hyperclass consists of an indefinite number of bags, each of which contains a ratio of α objects to β objects equal to the α-parameter (the probability value) assigned by the statistical hypothesis.

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[1] Braithwaite, op. cit., p. 118

[2] Ibid., p. 121-2

[3] Ibid., p. 130

Braithwaite treats probability as a theoretical concept which is employed only in statistical hypotheses; to understand probability, then, we must understand how statistical hypotheses are used, and what they mean. Braithwaite accepts a modified verifiability theory of meaning, in terms of which the meaning of a statistical hypothesis is given by means of a ' *k*-rule-of-rejection ', which directs us to reject a probability statement when confronted by evidence of a certain specifiable statistical character.

Consider a class β, a certain proportion of whose members are also members of α. Let there be a statistical hypothesis asserting that this proportion is p, i.e. that the α-parameter of any (α, β)-probability hyperclass has the value p. We draw an n-fold sample of β's, which corresponds, in Braithwaite's model, with a selection of one ball from each of n bags, each having the α-parameter p. Given an arbitrary small number k, we are directed by the k-rule-of-rejection to reject the hypothesis if the α-ratio in the sample differs from p by more than $\sqrt{pq/nk}$, where $q = 1 - p$.

There is an argument for using this rule of rejection. It is a theorem in the arithmetic of class ratios that the proportion of selections from a probability hyperclass whose α-parameter is p, which have α-ratios differing by more than $\sqrt{pq/nk}$ from p, is less than k. Therefore, if our statistical hypothesis is true, the probability that we shall reject it is less than k. This last probability statement is also a statistical hypothesis; it concerns the proportion of n-fold samples which have α-ratios lying within fixed limits, and it may be tested by means of a higher-level k-rule-of-rejection. We make n_1 sets of n observations each; if the proportion of n-fold samples which have an α-ratio lying outside of the fixed limits $p \pm \sqrt{pq/nk}$ is greater than $k + \sqrt{1/n_1}$, then we reject the second-level hypothesis, and therefore also the first-level hypothesis. If the first hypothesis was rejected in accordance with the k-rule, but the second one is not, then we may re-instate the first hypothesis on the grounds that its rejection was atypical. This same procedure may be applied to third-level hypotheses, fourth-level hypotheses, etc., each of which has its derived k-rule-of-rejection.

It may immediately be seen from this programme that a statistical hypothesis can never be definitely rejected or confirmed. 'Although every rejection may be cancelled, yet it also may not be; and the possibility of an unending series of empirical tests, none of which reject the statement serves to give, I maintain, an empirical meaning to the statement'.[1] It should be noted that on Braithwaite's theory the *k*-rule-of-rejection does not give us any *reason* for believing or disbelieving, accepting or rejecting, a statistical hypothesis. 'The reason for disbelieving a probability statement is the empirical[2] knowledge on the basis of which it has to be rejected in accordance with the *k*-rule-of-rejection which determines the meaning of the statement; *that it has to be rejected on the basis of such knowledge is not the reason for disbelieving it, but is a statement of how probability statements are used*'.[3] When *k* appears in the *k*-rule, it simply helps to characterise the kinds of observations which will result in the rejection of the original hypothesis. When *k* appears in the explanation of *why* we use a certain *k*-rule, then it is merely a parameter in a new hypothesis.

4

The discussion in *Scientific Explanation* of the confirmation of statistical hypotheses is much more detailed than the discussion of universal inductions which are confirmed by inductive policies. It is more important, in any case, since if the problem of statistical induction can be solved, we can always consider universal generalisations as merely a special type of statistical induction, in which the parameter assigned to the model has the value 0 or 1. The theory of statistical induction may be taken as the explication of the policy of induction by simple enumeration, upon whose effectiveness our reasonable belief in the effectiveness of every other policy must depend.

Braithwaite points out that the *k*-rule-of-rejection does not provide us with a means of justifying statistical hypotheses. To fail to reject a statistical hypothesis is not necessarily to accept it. There is, however, a theory of statistical inference which appears to be closely related to the *k*-rule-of-rejection: Neyman's theory of confidence intervals.

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[1] Braithwaite, op. cit. p. 162

[2] In the second impression, 1955, and the American Library of Science edition 1956 of Professor Braithwaite's book the following correction was made :—

"empirical tests none of which reject the statement" should read "empirical tests each of which rejects the statement". Ed.

[3] Braithwaite, op. cit., p. 166–7. Italics mine.

Given a probability parameter p for an (α, β)-probability hyperclass, it is possible to calculate two numbers $r_1 = f_1(p)$ and $r_2 = f_2(p)$ such that the probability that an n-fold sample of β's will possess an α-ratio lying between r_1 and r_2 is equal to $1 - k$, where k is a preselected small number. (There are any number of pairs of numbers satisfying this criterion; but one pair is particularly attractive for the job we want it to do.) It is possible to transform these functions, according to Braithwaite, into two new functions, $p_1 = g_1(r)$ and $p_2 = g_2(r)$ in such a way that, ' . . . whatever be the value p of the probability parameter, the probability is $1 - k$ that this value p is included in the interval $[g_1(r), g_2(r)]$, where r is the α-ratio in a sample of n β-specimens '.[1] I suspect that Neyman would object to having the number $1 - k$ called a probability; he refers to it himself as a ' confidence coefficient '. Like the number k in the k-rule-of-rejection, the number $1 - k$ functions as a description of how probability statements are used; in a given instance, the parameter in question either does or does not fall within the indicated interval, and on Braithwaite's view, as on Neyman's there is no probability involved at all.

If we pass to a higher-level statistical hypothesis, we may suppose, as Neyman seems to suppose, that the method of confidence intervals can be justified by reference to the long run use of the method: if we select a number k and make a great number of inferences of the kind described, then our long-run frequency of success will be roughly $1 - k$. (This, of course, is an empirical truth, not a logical one, if it is true at all.) But with respect to a single experiment the value of k, whether it is 0·80 or 0·90 or 0·99, is of only descriptive significance. It does not constitute, or even form part of, a reason for rejecting or accepting any statistical hypothesis. We therefore have no solution to the problem of enumerative induction: we have no answer to the question, ' Why *ought* we to accept the results of our statistical inferences? ' It is simply an empirical fact that we do accept them, and the confidence coefficient merely characterises the pragmatic rule in accordance with which we have acted.

Braithwaite rejects the possibility of distinguishing between the degrees of belief we are entitled to have in different hypotheses. ' The view taken in this book—that the question of the reasonableness of an inductive belief is bound up with that of the validity of the inductive inferences by which it has been or could be derived—thus leaves no room for the comparison of the reasonableness of different inductive beliefs. If the inference is valid, incorporation of belief in its conclusion into a man's rational corpus is justified, and it is reasonable, having the old reasonable belief [in the evidence], to acquire the new one. There is no question of its being more or less reasonable to do so '.[1] 'The reasonable acceptance of a hypothesis is an all-or-none reaction '.[2]

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[1] Braithwaite, op. cit., p. 248

A confidence coefficient is to have only descriptive significance. But what *is* its descriptive significance? What is the difference between an all-or-none reaction based on a rule in which the fraction '80/100' appears and an all-or-none reaction based on a rule in which the fraction '99/100' appears? Ordinarily a hypothesis accepted on the basis of a confidence coefficient of 0·99 is considered in a slightly different light than is one accepted on the basis of an inference involving a confidence coefficient of 0·80; the latter is considered not quite as persuasive, as conclusive, as likely, as probable (in ordinary parlance) as the former. It seems natural to suppose that one is entitled to a greater degree of belief in the former than in the latter, or to hold the former with greater tenacity than the latter.

It is often true that we can *act* only on one hypothesis at a time, and that consequently we cannot always be agnostic, no matter how little evidence we have or how evenly balanced the evidence is on both sides of the question. But it is also true that the risk involved in acting on a given hypothesis often depends upon how well-confirmed that hypothesis is; and the more adequately confirmed the hypothesis is, the more confidence we are *rationally entitled* to feel in acting upon the assumption of its truth. In this sense, it is clearly possible to distinguish between different degrees of rationally justifiable belief. And it is precisely this distinction that different confidence coefficients are intended to express.

Braithwaite confuses a scale of degrees of reasonableness of belief with the quite different scale of degrees of rational belief (degrees of belief of the rational kind), which Keynes and others have had in mind. He thinks that a scale of degrees of reasonableness of belief is a philosopher's myth, but he says, ' . . . although I am unable to recognize differences in the reasonableness of the beliefs which are in my rational corpus, I am able to recognize differences in the *tenacity* with which I hold them '.[3] The question of rationality may be brought up in this context as well as in the context of Keynes's discussion of degrees of belief; it is perfectly possible to ask, 'Is it *rational* to hold this belief of yours so tenaciously on such little evidence?' This is precisely the equivalent of the question that Keynes and others who have spoken about degrees of rational belief were attempting to consider.

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[1] Braithwaite, op. cit., p. 357

[2] Ibid., p. 355

[3] Ibid., p. 359

To employ Braithwaite's language, we may say that there is nothing unplausible about supposing that a standard can be formulated which will allow us to distinguish between *rational* and *irrational* degrees of tenacity in given circumstances. On the same evidence, are we not entitled to have a greater degree of belief in the hypothesis that between 40 and 60 per cent. of A's are B's than in the just-acceptable hypothesis that between 45 and 55 per cent. of the A's are B's (or to hold the former hypothesis with greater tenacity than the latter)? And it is tempting to suppose that these rational degrees of tenacity (or rational degrees of belief) are not unrelated, at least where statistical hypotheses are concerned, to the confidence coefficients that appear in the descriptions of what we have done in accepting the hypotheses.

5

There is a further limitation in Braithwaite's treatment of probability and statistical inference which bears heavily on the policy of induction by enumeration. He eschews any mention of randomness in the description of his model, or in his discussion of the relationship between his model and the part of experience which his model is supposed to reflect.

> The truth is that the theory of probability to which the balls-in-bags model has to correspond does not require the selections from the bags to be limited in any way. For what corresponds to the conclusion in the deductive system of the theory are propositions . . . about the proportions among the possible selections of those selections which have certain properties. And that in calculating these proportions each of the possible selections must be counted once and once only is a proposition which is true by virtue of the definition of 'proportion'. So it is otiose, and, indeed, misleading to speak of the selections or samples or draws as being 'random', or as being made from a class of 'equally probable' selections or samples or draws, since the equality of treatment which these epithets demand has already been provided by the set-up of the model.[1]

To illustrate the difficulties (or oddities) to which this disavowal of the usual randomness requirement might lead, let us consider an example. Let there be a (physical) bag containing ten (physical) balls, of which five are black and have iron cores, and five are white and have lead cores. Suppose that an investigator draws a fairly large number of balls (replacing each before drawing the next), and finds that about two-thirds of the draws result in a black ball. The model which he accepts (on the basis of a given value of *k*) consists of a large number of (theoretical) bags, each containing two (theoretical) black balls and one (theoretical) white ball. A draw of *n* balls from the physical bag will correspond to a selection from *n* of the theoretical bags of the model. The explanation of the probability parameter of two-thirds is that we have supplied our investigator with a (secretly) magnetised grapple, which is just twice as likely to grab an iron-cored ball as a lead-cored one.

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[1] Braithwaite, op. cit., p. 133

Now suppose we get another experimenter to conduct a similar experiment. We give him a non-magnetic grapple (otherwise just like the one used in the first experiment), and he goes through the same procedure as the first man. He will come up with a model consisting of bags each containing two balls, one black and one white; the probability parameter in his hypotheses will be 1/2.

We might expect the following dialogue to ensue:

A: 'I got a parameter of 0·5; is that what you got?'
B: '0·5? Impossible! I got one of 0·67.'
A: 'There's something very odd going on here.'
B: 'You said it! Say, let's trade grapples and see what happens; maybe that has something to do with it.'

But on Braithwaite's interpretation of statistical hypotheses, there is nothing odd about the situation at all. Where randomness is not involved, neither is bias. In each case it is perfectly plausible to assume that there is a statistical hypothesis, and since each hypothesis only asserts something about the class of samples *drawn* from the parent population, and nothing about the parent population itself, there is no reason why both A's hypothesis and B's hypothesis should not be accepted. The dialogue must run:

A: 'I got a parameter of 0·5; is that about what you got?'
B: 'No, I got a parameter of 0·67.'
A: 'Splendid, old man. We only expected to come up with one hypothesis, and now it looks like we've got two. We can both publish!'

If we are to accept both hypotheses, we must distinguish between them; the most plausible way is to mention the method of drawing samples to which the hypothesis applies. Our investigators, not knowing about the magnetic grapple, will only be able to say that one hypothesis applies to 'A's method' and the other applies to 'B's method'. From this example, it is clear that the hypothesis asserts only that *the samples drawn from a parent population by a certain method* correspond to a selection from a probability hyperclass which has a certain parameter. A statistical hypothesis tells us something about the samples drawn by a given method and from a given population, but not about the composition of the population itself.

Now suppose the experimenter with the magnetic grapple draws more balls at a time; if he draws three balls at a time the probability parameter of his statistical hypothesis will be 0·65 rather than 0·67; if he draws ten balls at a time, before replacing them, the parameter will be 0·5. It is clear that if the population is finite, we should include the size of the sample as well as the method of drawing it in the statement of the statistical hypothesis.

The value of the parameter p in the statistical hypothesis which will never be rejected by the k-rule-of-rejection, if there is such a value, is dependent on (*a*) the method of drawing the samples, and (*b*) the size of the samples which are drawn between replacements, if the population is finite, as well as (*c*) the constitution of the population being sampled.

6

Braithwaite's attempt to justify the inductive policy of induction by simple enumeration was not satisfactory; one argument established that if it were in fact a valid policy, then we could reasonably believe in it, but did not establish that it was in fact a valid policy, and hence did not establish that we could in fact reasonably believe in it. The other argument was so weak that it could be used as well to justify the prediction of the future from tea-leaves as to justify the policy of induction by simple enumeration. From this failure, it follows that we cannot (yet) justify the other inductive policies by reference to the policy of induction by simple enumeration.

We may also, I think, drop the reference to inductive policies altogether without altering the picture; the empirical element which is essential to the statement of the effectiveness of an inductive policy can be expressed as an ordinary empirical hypothesis, and we may of course use well-tested empirical hypotheses in arguments leading to new inductive conclusions. The basic difficulty seems to lie in the justification of induction by simple enumeration.

As Braithwaite points out, the policy of induction by simple enumeration can be considered merely a special case of the general policy of making statistical inductions. If we can show that the argument from a sample to a population is *reasonable*, we will have shown that induction by simple enumeration is reasonable; and if induction by simple enumeration is reasonable, then it can be used to confirm hypotheses and theories which in turn may be employed in future arguments.

Braithwaite's treatment of probability and of statistical hypotheses does not seem to be adequate to the justification of induction by enumeration, however. The mysterious *k* which appears both in the elucidation of the meaning of statistical hypotheses and in the procedure for testing statistical hypotheses, represents merely a part of the description of how probability statements (statistical hypotheses) are *used*; it does not tell us why they are or ought to be used that way, and it provides no *justification* for accepting a hypothesis which will pass the confirmation test we have chosen for it. In particular, on Braithwaite's theory, there seems to be no reason for choosing a test employing one confidence coefficient rather than another. If a ' reason ' is offered, it must be construed as another statistical hypothesis, subject, like the original hypothesis, to confirmation or refutation.

Braithwaite's conception of beliefs as either reasonable or unreasonable, regardless of the degree of tenacity with which they are held, while it is in conformity with his cavalier treatment of confidence coefficients, does not seem plausible. While it may be ' reasonable ' to believe the statistical hypothesis that very close to nine-tenths of the electrons fired at a target in a particular experiment will hit it, it is not so clearly ' reasonable ' to believe that a particular fall of a die will result in some side other than the one-spot, and although we may be forced to act on one assumption or the other, it is difficult to know whether on a toss of a coin belief in the occurrence of heads or in the occurrence of tails would be ' reasonable '. We are often concerned with general statistical hypotheses (e.g. that about half of all coin tosses result in heads) simply because they provide grounds for determining the appropriate (rational) degrees of partial belief in particular instances.

Finally, the attempt to do without randomness results in some unusual limitations in the scope of statistical hypotheses: (1) the hypothesis applies only to a specific method M employed in obtaining the samples on the basis of which it is confirmed or rejected; (2) the hypothesis only applies to a specific sample size (between replacements) unless the parent population is infinite; and (3) the hypothesis can only refer to the composition of the samples drawn (of the given size and by the given method) and does not generally give us any non-hypothetical information about the composition of the parent population. In order to pass from knowledge of the composition of the samples to knowledge of the composition of the parent population, some statement about the randomness of the sample must be employed.[1]

While it may be possible to accept these restrictions, most statistical assertions do not conform to them. In most branches of science it is felt that the data obtained from samples give some information about the parent population from which the samples were drawn. On Braithwaite's interpretation there is no need to be concerned about randomness or bias; the more discordant results that are obtained, the more valid statistical hypotheses there are. But statisticians are often very much concerned with randomness, and they do not always seem to treat an assertion about randomness as an empirical hypothesis to be confirmed like any other. In many cases it is clear that any attempt to confirm such an hypothesis would be absurd. There is a considerable discrepancy between the theory proposed by Braithwaite and the practice of statisticians.

This is not the place for me to present an alternative to Braithwaite's theory of probability.[2] I have indicated that Braithwaite's theory of induction is unsatisfactory when it comes to the fundamental form of induction—the confirmation of statistical hypotheses. I think that the difficulties in his treatment of the confirmation of statistical hypotheses stem from an inadequacy of his theory of probability, and in particular from a model interpretation of probability which does not take into account the serious problems of interpreting randomness. Given a definition of randomness which allows us to relate statistical probabilities to degrees of belief, we can overcome these difficulties. It is precisely in this relation that a solution to the problem of induction must be sought.

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[1] This need *not* be an empirical statement to the effect that the method draws each possible sample equally often—it may be a merely logical statement to the effect that we have no grounds (logical or empirical) in our rational corpora for supposing the method to be biased.

[2] I have in process a rather lengthy treatise, *Probability and the Logic of Rational Belief*, which does present an alternative treatment of probability.

The general scheme for the justification of induction presented in *Scientific Explanation* still seems appealing and valid. If we can establish the reasonableness of making statistical inductions (or a framework for evaluating the degree of belief in a statistical hypothesis which is warranted by the evidence for the hypothesis), then we can proceed in accordance with Braithwaite's scheme. Braithwaite's major contribution to the problem of induction consists in the fact that he focuses his attention on the central problem of confirming statistical hypotheses where his predecessors had erroneously concentrated on the problem of confirming universal generalisations.

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DISCUSSIONS

On Mr Roy Harrod's New Argument for Induction

In Mr Roy Harrod's book *Foundations of Inductive Logic*, ably reviewed by Dr Bronowski in this *Journal*, there occurs an argument which is both completely new and crucial for the book. I have talked about this argument with a number of people, knowledgeable in this field, and they all reacted in the same way. They were all convinced that the argument was invalid ; they all could give good and sufficient reasons why it *had to be* invalid ; but they could not pin-point the mistake. They rightly remarked that Mr Harrod expresses himself very vaguely at the decisive places ; but I was not convinced that this remark was a perfect substitute for pin-pointing the mistake. For Mr Harrod's crucial argument is completely new ; it is a bold argument and shows much ingenuity. I felt, for this reason, that an attempt to show, not so much why Mr Harrod's argument *must be* invalid, but rather why it *is* invalid, may perhaps be not quite superfluous.

Mr Harrod tries to establish a proposition which may be formulated as follows.

If we travel in a vehicle through a desert which we have entered at zero minutes, then there is always, at m minutes a probability equal to 0·99 of not reaching the end of the desert at $m + (m/99)$ minutes ; or more generally, there is a probability equal to $x/(x + 1)$ of our not reaching the end at $m + (m/x)$ minutes.

Now since this holds for every number m, we can say that there is always a very high probability, $p = x/(x + 1)$, of something that has been going on for some period of time to go on for just a little longer ; which does not sound unreasonable.

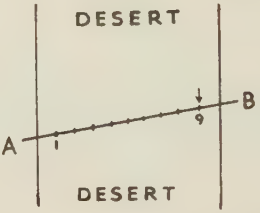
This, I think, is the crucial proposition Mr Harrod wishes to prove. His argument is quite simple, and might be expressed, a little more precisely, in *the language of betting*, as follows.

Assume that two gamblers, Peter and Paul, agree on the following betting contract. It is assumed that at any minute m from the moment of entry into the desert, a bet is made according to which Peter owes Paul $x/(x + 1)$ pennies if the end of the desert is reached within the next m/x minutes. If the end is not reached within this period of time, then Paul owes Peter $1/(x + 1)$ pennies. After reaching the end of the desert, no further bet is made, and the debts are settled.

It can easily be shown that, upon the assumption that the units chosen—the ' minutes '—are small as compared with the total time of crossing the desert, this contract is a *fair betting contract*, in the sense that, upon conclusion of the contract and settling of the debts, it will be found that neither of the two gamblers owes the other gambler anything. Now this means that the odds of x to 1 in favour of not reaching the end during the next period of time were *fair odds* ; which means, further, that the probability p of not reaching the end of the desert within that period is $x/(x + 1)$.

That the betting contract is fair may be shown, following Mr Harrod, by the following diagram (in which we assume that we proceed with constant velocity—an assumption which need not be challenged).

The path from A to B is our way through the desert. We freely choose a number x—say, $x = 9$, for the purpose of this diagram—and divide the distance from A to B into $x + 1$ parts (i.e. 10 parts). The arrow in our diagram points to the beginning of the last (10th) part. It is clear that Peter's bet that we shall not reach B within the period of time m/x will be won by Peter anywhere before the arrow, and thus in nine cases out of ten.



Accordingly, there is at every minute m a probability of $p = x/(x + 1)$ of not reaching the end of the desert ; which seems to prove Mr Harrod's proposition.

This, I believe, is Mr Harrod's argument, restated in terms of a gambling contract.

A slight variant would be the following. Instead of contracting (as in our '*first contract*') to gamble at every minute (or some other unit of time), the parties may agree on a '*second contract*' according to which they gamble only once (or twice . . .), at an instant of time m determined by drawing a ball (or two balls . . .) from an urn, before entering the desert. If the ball shows the number m, then, according to the contract, the game is played at m minutes, if the desert is not at an end at m minutes ; and should it so happen that the end of the desert is reached before these m minutes have elapsed, then the contract lapses, and no game is played.

If the game is played, then the odds are determined as before : there will be fair odds of x to 1, or a probability of $p = x/(x + 1)$, in favour of not reaching the end of the desert within the next m/x minutes.

Now these calculations can all be easily checked, and they are correct. What, therefore, is wrong with Mr Harrod's argument ?

My reply is this. Simple as this argument is, it is sufficiently complicated to make its verbal formulation not too easy. Accordingly, it is not quite easy to say in two words *what the event is* whose probability p we have calculated as $p = x/(x + 1)$. It is through this verbal complexity that the mistake creeps in.

Mr Harrod suggests that if I travel through the desert, knowing that I entered it m minutes ago, but not knowing when it will end, then there will be a probability of $x/(x+1)$—say of 99/100—of *the event of* '*not reaching the end of the desert during the next m/99 minutes*'.

But this interpretation of the meaning of p can be easily shown to be mistaken, with the help of the following simple proposition :

The gambling contracts described remain fair, even if either Peter or Paul or both know exactly when the end of the desert will be reached (for example, at 60 minutes).

That this is so is clear from the generality of the arrangement : from the fact that the contracts are *generally* fair—that all the conditions assuming fairness are incorporated in the contract and that lack of knowledge was not one of the stipulated conditions.

It follows from this that the probability p which Mr Harrod *correctly* calculates *cannot be a probability that may be interpreted as the degree of Peter's or Paul's imperfect knowledge.*

This is all that is needed to answer Mr Harrod. Thus the complete mathematical generality of his model establishes the fact that he misinterpreted his result.

If the question is asked, What is p ? What is the event whose probability equals p ? then the answer will be a little different according to the two contracts.

Under the *first contract*, p is nothing but the proportion of the number of games which Peter is bound to win to the total number of games to be played under the contract.

Under the *second contract*, p is the classical probability of Peter winning his gamble: it may be large because it is unlikely that we shall draw a number placing the bet just immediately before the end of the desert. Mr Harrod expresses this by saying that it is unlikely that we shall find ourselves just at the edge of the desert. This is correct : it is unlikely that we shall find ourselves, *by accident*, close to any *given spot*; for example, within a unit from the end of the desert—*or from its beginning*. But this has nothing whatever to do with our knowledge, or our lack of knowledge, of where that spot is ; and it therefore must not be interpreted as a measure of the completeness or incompleteness of that knowledge.

It may be helpful to discuss an objection to my analysis. My argument, it may rightly be said, depends upon Peter and Paul *entering into unalterable contract*. Clearly, once they have entered into an unalterable contract, new knowledge—such as information concerning the moment at which the end of the desert will be reached—cannot make any difference. But if we allow Peter, or Paul, to *cancel* the contract—for example, the first contract—then any superior knowledge possessed by the one or by the other will enable him to win.

My reply is this. If we permit Peter to cancel the first contract (exactly analogous remarks hold for the second contract), then this is *sufficient* to make it an *unfair* betting contract. For Peter can now, *without any further knowledge*, improve his chances of winning ; for example, by cancelling the contract *always* after the first bet. Since we have to assume in any case that the units of time—that is to say, the distances between the bets—will be small as compared with the total travelling time across the desert, the policy of cancelling *after* the first bet would mean that Peter can win his bet regularly. (If Paul has the right of cancelling, he would, accordingly, always cancel the first bet itself ; so that there would never be a game if both can cancel.) We thus see that it is essential to the fairness of the betting contract that neither Peter nor Paul has the right to cancel it. Accordingly, Peter's (or Paul's) foreknowledge of the end of the journey through the desert cannot influence the game at all (provided the game was fair) ; and consequently, the probability p must not be interpreted as a probability that may be ascribed to an uncertain hypothesis concerning the future arrival at the end of the desert. Thus p is completely different, in this respect, to the probability, say, of betting on heads or tails in an ordinary game ; for here the probability remains $\frac{1}{2}$, even if we allow either party to cancel the bet beforehand ; and in this way, foreknowledge of the result possessed by one party would increase the probability of a win by this party from $\frac{1}{2}$ to 1. (Of course, any properly calculated *probability* is always that of a *fair* gamble.)

In conclusion, a simple paradox may be presented that appears to be fatal for Mr Harrod's inductivist interpretation of his probabilities.

We slowly travel along a row of equally spaced telegraph posts. Mr Harrod's argument allows us to say (by choosing an appropriate x) that there is a probability as great as we like that this type of row will continue for a distance of at least $1/x$ of the stretch it has so far continued ; from which we will be able to conclude that there is a very high probability of encountering the next telegraph posts within, say, one half of a second.

But Mr Harrod's argument is applicable to any kind of stretch or gap, allowing us to predict, with arbitrarily high probability, that the stretch or gap in question will continue a little longer. It is thus applicable to the gaps between any two telegraph poles, and it may yield a very high probability that we shall *not* encounter the next telegraph pole within one half of a second. Yet these two probabilities contradict each other.

Our own solution is obvious : the odds refer to entirely different gambling contracts (to different *relative* probabilities). Yet the paradox establishes that Mr Harrod's always applicable method of calculation cannot possibly be interpreted as referring to inductive probabilities. It is not his mathematics that is at fault but his interpretation ; and to avoid misinterpretation of this kind, one of the best general methods is to state one's argument in terms of an appropriate gambling contract.

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'Philosophical' and 'Scientific' Sense-Data

In a recent review of my book in this journal Mr Tucker draws a distinction between 'philosophical' and 'scientific' sense-data on the grounds that they are different kinds of *entity*. This distinction seems to be in error. Sense-data, as I have defined them, are empirical entities and philosophers such as Russell, Moore, Broad, and Price are clearly spending much of their time talking about 'scientific' sense-data. Moore[1] defines sense-data in a way very similar to mine with reference to after-images and the others clearly use the term to refer to the empirical entities that they find available to introspection in their visual fields (see e.g. Broad[2]). Having noticed and described these empirical entities, it is then possible to make epistemological analyses such as 'Only statements about sense-data are certainly true' or 'Our knowledge about physical objects is based on an acquaintance with sense-data', etc. But it is completely misleading to claim that the 'philosophical' sense-data and the 'scientific' sense-data are different kinds of *entity*. When we say 'Our knowledge about objects is based on our acquaintance with sense-data' we are talking epistemologically about 'scientific' sense-data and not about something else called 'philosophical' sense-data. Sense-data are empirical entities which have certain kinds of relations, to minds and to physical objects, that give rise to epistemological arguments about certainty, etc. But the sense-data mentioned in these epistemological arguments, or even defined epistemologically,[1] are not some kind of shadowy entity over and above the empirical sense-data. Of course we can also discuss the *concept* of sense-data, but the only wits we have to keep about us here are those that we normally use to distinguish between entities, our concepts about these entities and the words that we use to express these concepts and to denote these entities. When Broad and Moore ask what is the relation R between sense-data and physical objects they are not discussing the relation between our concepts of sense-data and our concepts of physical objects, but they are discussing the relation between sense-data and physical objects. To say that ' philosophical sense-datum theories are about epistemological certainty ' implying that they are not about empirical sense-data is like saying ' A jury's verdict is about certainty ' implying that it is not about murder. So far from my being deceived by puns, Tucker is the victim of a category mistake that one would have supposed that so close a follower of Professor Ryle could have avoided more easily.

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[1] G. E. Moore, 'Visual sense-data', *British Philosophy in the Mid-Century* (C. A. Mace, Ed.), London, 1957

[2] C. D. Broad, *Religion, Philosophy and Psychical Research*, London, 1953, pp. 32 and 37

J. R. SMYTHIES

MULTIDIMENSIONAL TIME

I HAVE the following comments to offer upon Dr Mario Bunge's note in which [2] he refers to earlier papers by myself on the subject of two-dimensional time.

(i) I have not tried to import *psychological* implications into mathematical expressions occurring in physical theories. I have only attempted to indicate some logical consequences of postulating a parallelism between certain psychological and physical facts.

(ii) I have not suggested that there is anything ' mysterious ' or ' unreal ' in connection with complex variables, or with the pure imaginary parts of complex variables. On the contrary, I have been at pains to emphasise that, if one does introduce a second time dimension, as the pure imaginary part of a complex time variable, this pure imaginary time must be given full physical significance.

(iii) The essential point of the second sentence of the last paragraph of Bunge's note, if I have understood it, is that where complex variables enter into the mathematical expression of physical theories, both the ' real ' and ' pure imaginary ' parts of such variables not only *can*, but *must*, have a physical ' correlate ' (i.e. an identifiable referent in the physical world). That both parts of a complex variable *can* have full physical significance in this sense is, of course, well known to anyone who is acquainted with the elementary theory of electricity, where a particularly simple example is to be found in the so-called ' impedance ' of a circuit. Not only do both the real and pure imaginary parts of the complex quantity called ' impedance ' have physical correlates, but both can be separately measured. However, at the Quantum-mechanical level of physical phenomena the matter is not so simple because, in general, as has been emphasised by Dirac ' No meaning can be attached to the measurement of a complex observable '. Dirac points out[1] that ' It would not do to measure separately the real and pure imaginary parts because this would mean two measurements which in general would interfere with one another '.

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[1] E.g. ' whatever we can be certainly said to perceive ', that is in terms of incorrigibility: this still *refers*, however, to empirical sense-data.

[2] This *Journal*, 1958, **9**, 39

(iv) Bunge omits all mention of the troublesome considerations which physicists themselves call ' reality conditions '. Indeed he seems to overlook ' reality conditions ' altogether when he claims that ' complex quantities entering physical theories are—provided they do not play a barely intermediary syntactical rôle—just pairs of real quantities having each a physical correlate '. This statement is *not* true in the case of those physical theories in which ' reality conditions ' are imposed in order to deprive certain complex quantities of physical significance, without thereby relegating them to a ' barely intermediary syntactical rôle '. A well-known example of such reality conditions is Dirac's postulate, restricting the representatives of physical observables to those Hermitian operators which satisfy his expansion theorem, with the corollary that an arbitrary eigen state of a physical observable is dependent upon the eigen states of Hermitian operators. In claiming that Dirac's reality conditions do not come within the scope of Bunge's remarks I admit that I am not clear as to the exact meaning of his phrase ' a barely intermediary syntactical rôle ', in connection with the question of the physical interpretation of mathematical expressions. However it would seem that a clear example of a mathematical quantity which plays a ' barely intermediary syntactical rôle ' is Dirac's improper δ-function. This δ-function never appears in a final result except as a factor in an integrand. Consequently, as Dirac says,[2] ' The use of [such] improper functions is . . . a convenient notation enabling us to express in a concise form certain fundamental formulas [*sic*] which we could, if necessary, rewrite in a rigorous form, but only in a cumbersome way in which the parallelism with the case of discrete eigen values would be obscured '. Thus Dirac's improper δ-function does clearly play a ' barely intermediary syntactical rôle '. But it is equally clear that this rôle is different from that played by the complex, non-diagonal, elements of matrices representing ' observables ' in quantum mechanics : these latter quantities could not be eliminated merely by rewriting the mathematical expressions ' in a cumbersome way '. Thus complex quantities, subject to ' reality conditions ', can in general neither be regarded as ' playing a barely intermediary syntactical rôle ', nor as just ' pairs of real quantities each having a physical correlate '.

Despite these criticisms of Dr Bunge's note I find it interesting to learn from him that theoretical physicists are now finding it convenient to use the language of two-dimensional time to express theories of ' sub-nuclear phenomena ' (i.e. phenomena whose explanation involves the internal structure of the ' elementary particles ' of matter). Theories of sub-nuclear structure are certainly required to bring order into the jungle of observed facts about the ' elementary particles ' currently postulated by theoretical physics. For example, it is very probable that a satisfactory theory of the magnetic moment of the neutron will require a hypothesis ascribing to the neutron an internal structure comprising an equal number of spinning electrical point charges of opposite sign (chirality), whose motions are not symmetrical with respect to the centroid of the system. (Such lack of symmetry in its internal structure might well be the clue to the problem of the instability of the free neutron.) A model of this kind can be constructed, using Bunge's complex time-operator, in such a way that spinning charges of opposite electrical sign (chirality) have components of momentum with opposite sign with respect to the pure imaginary time. Thus it becomes possible for pairs of charges of opposite sign (chirality) to cohabit the same world-line in an electrically neutral region of ordinary 4-dimensional space-time, while having different 5th dimensional components. So we get a picture of pair production in which ' anti-particles ' (i.e. particles of opposite chirality) are treated as particles of the same nature with opposite components in the 5th dimension. Prior to pair production anti-particles can be conceived as initially cohabiting the same world-line in ordinary (electrically neutral) 4-dimensional space-time, but as having opposite 5th dimensional components with respect to the imaginary time dimension, which cancel each other. An initially neutral system of this kind can break down into two chirally opposite particles, following different world-lines in 4-dimensional space-time, whenever the system is subject to a perturbation of sufficient energy. When, for example, a positron and an electron are produced from the interaction of a gamma ray photon with a magnetic field in a Wilson Cloud Chamber, what happens, according to this picture, is the splitting of an electrically neutral world-line into two different loci, which have components of momentum of opposite sign with respect to the 5th dimension. Similarly, the annihilation of two anti-particles on collision is depicted as the conflation of two loci, each of which has, initially, a 5th dimensional component of momentum of opposite sign with respect to the pure imaginary part of the complex time, to yield a world-line with no resultant component of momentum with respect to the imaginary time dimension.

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[1] *Quantum Mechanics*, 2nd edn., 1935, p. 29 n.

[2] Ibid., p. 74

A satisfactory theory of pair production and pair annihilation is only one of the advantages furnished by the adoption of a five dimensional theory, employing a complex time, in which both the real and pure imaginary parts are given full physical significance.

H. A. C. Dobbs

The Paradoxes of Confirmation

An interesting article[1] by Mr J. W. N. Watkins has given rise very recently to an exchange of papers between him and Professor Hempel.[2] Both Watkins's original article and the further papers are chiefly concerned with the problem of providing a general criterion for distinguishing empirical from non-empirical statements. One of the topics discussed at some length is the problem raised by Hempel's ' paradoxes of confirmation '.[1] In this note I shall first try to show that Watkins is mistaken in claiming that the paradoxes provide a conclusive argument in support of his position and then I shall attempt to explain how the paradoxes arise.

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[1] Watkins, ' Between Analytic and Empirical ', *Philosophy*, 1957, **33**, 112-131

[2] Hempel, ' Empirical Statements and Falsifiability ', *Philosophy*, 1958, **34**, 342-8; Watkins, ' A Rejoinder to Professor Hempel's Reply ', *Philosophy*, 1958, **34**, 349-55.

I

In order to give an account of the paradoxes, one cannot do better than quote Hempel's own summary (Hempel, 1958, p. 344).

> It seems reasonable to consider an object, say b, as confirming a hypothesis of universal conditional form, such as ' $(x)[\text{Raven}\ (x) \supset \text{Black}\ (x)]$ ', if b satisfies the antecedent and the consequent of the conditional, i.e. if b is a raven and is black. It also seems reasonable to agree that an object confirming a given hypothesis confirms as well any statement logically equivalent to it. Now, the hypothesis just mentioned is equivalent to the statements ' $(x)\ [\sim \text{Black}\ (x) \supset \sim \text{Raven}\ (x)]$ ' and ' $(x)\ ([\text{Black}\ (x) \vee \sim \text{Black}\ (x)] \supset [\sim \text{Raven}\ (x) \vee \text{Black}\ (x)])$ ', the former of which is confirmed by anything that is neither black nor a raven, the latter by anything that is not a raven or is black. Hence any object of either of these types must be qualified as confirming the hypothesis that all ravens are black.

Hempel accepts this conclusion and in his original article argues that it only appears paradoxical because we do not normally consider hypotheses in isolation but only in the context of an existing body of presuppositions. ' If we are careful to avoid this reference to additional knowledge . . . and if we formulate the question as to the confirming character of the evidence in a manner adequate to the concept of confirmation as used in this paper . . . the paradoxes vanish ' (Hempel, 1958, p. 20).

Watkins, on the other hand, draws the conclusion that we should abandon the notion of confirmation through observation of instances and should instead follow Popper in interpreting ' the confirmation of a testable hypothesis in terms of persistent and unsuccessful attempts to falsify it. . . . A confirmed hypothesis is a hypothesis which has " proved its mettle " by withstanding severe tests ' (Watkins, 1957, p. 117). And in his latest paper he writes (p. 351) :

> On a Popperian theory of confirmation this hypothesis is confirmed by an observation-report of a black raven, not because this reports an instance of the hypothesis —a white swan is also an instance of it—but because it reports a satisfactory test of the hypothesis : a raven has been examined unsuccessfully for non-blackness. Statements about non-ravens which do not report tests of our hypothesis cannot confirm it.

He also points out that it seems very strange to accept a theory of confirmation according to which a hypothesis can be confirmed equally by two such flatly contradictory statements as ' *a* is a cat and not black ' and ' *a* is no cat and black '.

One must admit that Watkins is in general right in stressing the importance of Popper's falsifiability criterion. As applied to scientific theories it is clearly fundamental. But it is not so obvious that the criterion is applicable in exactly the same way and with exactly the same force when one is considering simple generalisations such as ' All ravens are black ', especially when these are taken in an artificial isolation. One possible difficulty is that Watkins, having rejected the notion of confirmation by observation of instances at the first level, may find himself re-introducing it at the next. For he speaks of confirmation of a hypothesis ' in terms of persistent and unsuccessful attempts to falsify it '. But this seems to suggest an investigator arguing, ' Many individual efforts to falsify this hypothesis have failed ; therefore it is highly probable that all subsequent efforts will fail '. It seems that each of the unsuccessful efforts will count as confirmation of the second level hypothesis which states that the first level hypothesis will never be falsified. This objection does not, of course, bear directly on the paradoxes ; in fact, if one tried to consider the second-level hypothesis in isolation the paradoxes would arise again at this next stage. What it does seem to show is that the notion of instantial confirmation is just as basic as that of falsifiability, since the rationale of the latter cannot be explained without using the former notion—the alternative of explaining the apparent confirmation of the second-level hypothesis in terms of unsuccessful attempts to falsify a third-level hypothesis being clearly unacceptable since it would start one on an infinite regress.

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[1] See Hempel, ' Studies in the Logic of Confirmation', *Mind*, 1945, **54**, 1-26 and 97-121

In the present context, a far more important objection to Watkins is that his suggestion does not in fact help us to avoid the paradoxes. He implies that they only arise if we talk of confirmation, and that if instead we spoke only of attempts to falsify the paradoxes would disappear. But in order to show this he will have to explain more fully the notion of ' attempts to falsify ' or ' tests of the hypothesis '. For here also we must presumably accept an equivalence condition such as : whatever counts as an attempt to falsify (or as a test of) a hypothesis p must count as an attempt to falsify (or a test of) any hypothesis which is logically equivalent to p. But if we accept this condition then whatever counts as an attempt to falsify ' All non-black things are non-ravens ' will count as an attempt to falsify ' All ravens are black '. The presupposition of Hempel's discussion is that we must consider only the hypothesis and not introduce tacit reference to any of our existing knowledge about ravens, black objects, etc. If we accept this limitation then there in general seems no reason why the finding of black shoes (there *might* have been a non-black raven in the closet) or of white swans (there *might* have been some non-black ravens in the swannery) should not count as unsuccessful attempts to falsify ' $(x)[\sim \text{Raven}(x) \vee \text{Black}(x)]$ ' and therefore as unsuccessful attempts to falsify ' All ravens are black '.

The first conclusion I would draw from this is that Watkins does not succeed in solving or dissolving the paradoxes. The second, that the notion of confirmation through unsuccessful attempts at falsification will need explanation and modification if it is to be applied to simple generalisations ; for in this type of case it is by no means obvious what is to count as a test of the hypothesis.

2

If, then, the paradoxes occur with equal force if one accepts Watkins's position, one is compelled to look for some other explanation of them. The general form that such an explanation must have was outlined by Hempel in his original article, where he writes, ' In the seemingly paradoxical cases of confirmation we are often not judging the relation of the given evidence, E alone to the hypothesis H . . . ; instead, we tacitly introduce a comparison of H with a body of evidence which consists of E in conjunction with an additional amount of information which we happen to have at our disposal ' (Hempel, 1945, p. 20). But it is important to see exactly what is this additional amount of information which we usually have or presuppose that we have.

It is hardly enough just to accept the fact that the paradoxes disappear if we limit our evidence ; we also want an account which will explain why the paradoxes arise in the first place.

Hempel himself suggests that the extra evidence which we tacitly presuppose is the non-emptiness of the subject class—in his example we presuppose that there are ravens. He writes (Hempel, 1945, p. 17) :

> . . . if someone feels that objects of types *c* and *d* (*c* = black non-ravens, *d* = non-black non-ravens) are irrelevant rather than confirming . . . then this may mean that he is consciously or unconsciously construing the latter as having existential import ; and this kind of understanding of general hypotheses is in fact very common. In this case the ' paradox ' may be removed by pointing out that an adequate symbolization of the intended meaning requires the adjunction of an existential clause.

But this surely is wrong. For suppose we do interpret ' All ravens are black ' as ' (x) [Raven $(x) \supset$ Black (x)] . $(\exists x)$ [Raven (x)] ', then it is clear that observation of non-ravens cannot be said to confirm both parts of this hypothesis. But observation of one raven will *conclusively* confirm the second part of it. Thus after one raven has been observed we will only be interested in collecting evidence in support of the first part of the hypothesis and since, as we have seen, this is equivalent to ' (x) [$\sim$ Raven (x) v Black (x)] ', we shall again apparently have to admit white swans etc. as confirmatory evidence. Thus the paradox is not removed.

So we must look elsewhere to find what additional information is usually presupposed. An example of a situation in a closed group will help us. Suppose we are considering the hypothesis that all philosophy post-graduate students have I.Q.'s of more than 120 and let us suppose also that we already know independently that such students (ϕ) form 1 per cent of the total number of students and that there are exactly 20 per cent of students with I.Q.'s of more than 120 (ψ). Then if there were no correlation whatsoever between being ϕ and being ψ (i.e. being a philosophy post-graduate student and having an I.Q. of more than 120) we might expect the following distribution.

(a) $\phi \cdot \psi$	(b) $p \cdot \sim\psi$	(c) $\sim\phi \cdot \psi$	(d) $\sim\phi \cdot \sim\psi$
0·2 per cent	0·8 per cent	19·8 per cent	79·2 per cent

But if it were the case that all ϕ students were ψ, we should expect

1 per cent	—	19 per cent	80 per cent

Now if we simplify the situation still further so that we only have to choose between the hypothesis that all *p* objects ϕ objects are ψ and the hypothesis that there is no correlation at all, then it will be clear that *a* instances will provide more confirmation than *d* instances.[1] Two different but converging arguments can be used to show this. Firstly one can argue that if one finds say 10 *a* instances this would constitute very strong evidence for the hypothesis that all ϕ objects are ψ because the chances of finding 10 *a* instances and no *b* instances among 10 ϕ objects would, if there were no correlation, be very small—in my example $(\frac{1}{5})^{10}$. On the other hand, the chances of finding 10 *d* instances and no *b* instances among 10 $\sim\psi$ objects would be considerable—$(\frac{99}{100})^{10}$—even if there were no correlation. Secondly one can point to the fact that if the hypothesis is true the total number of *a* instances in the population will be 5 times greater than it would be if there were no correlation. Thus if the percentage of *a* instances in a random sample of the population is around 1 per cent or greater than 1 per cent it will make it more likely that the hypothesis that all ϕ objects are ψ is true. On the other hand, the percentage of *d* instances in this example will be almost the same whether or not the hypothesis is correct, and thus it will be impossible to draw any argument for or against the hypothesis solely from the proportion of *d* objects in a sample of the population. The same is true of *c* instances though, if anything, a higher percentage of these in a random sample would tend to be disconfirmatory.

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[1] The problem of assigning a probability value to a hypothesis on the basis of evidence from a sample is a difficult and controversial one, and many would agree with Kneale (*Probability and Induction*, pp. 211 f.) when he argues that it is impossible to assign exact numerical values to the probability of hypotheses. But if we are not prepared to accept a cardinal scale for degrees of confirmation, we must surely admit the possibility of saying in some cases that one parcel of evidence provides more confirmation for a hypothesis than another—three black ravens clearly provides more evidence for ' All ravens are black ' than one black raven.

Of these two arguments, the first is clearly more convincing and also nearer to the way in which we usually reason—and we are attempting to discover what are the assumptions made by us in our everyday arguments, the assumptions which make Hempel's argument appear so paradoxical. For the second argument would only be cogent if we made some more or less exact numerical estimates of the different frequencies involved, whereas the figures involved in the first argument are of such different orders of magnitude that any calculation is superfluous. It is true, of course, that the situation will be far more complicated if we do not just have to choose between the hypothesis that all ϕ objects are ψ and the hypothesis that there is no correlation. But even if we allow for the possibility of there being different degrees of positive correlation, it is clear that *a* instances will, in the absence of any extra information, provide better confirmation for the hypothesis that all ϕ objects are ψ than *d* instances.

On the other hand, it is comparatively easy to imagine a situation in which this would not be true. Suppose we are visiting an American campus where we know that 80 per cent of the male students are undergraduates (ϕ) and that 90 per cent of the male students have short hair (ψ). We then consider the hypothesis that all male undergraduates have short hair—that all ϕ objects are ψ. In this case the corresponding figures would be

(a) $\phi\cdot\psi$	(b) $\phi\cdot\sim\psi$	(c) $\sim\phi\cdot\psi$	(d) $\sim\phi\cdot\sim\psi$
72 per cent	8 per cent	18 per cent	2 per cent

if there is no correlation, while they could be

80 per cent	—	10 per cent	10 per cent

if all ϕ objects are ψ

In such a situation *d* instances would provide better confirmation than *a* instances—that is, we should be more profitably employed looking for long-haired graduate students than for short-haired undergraduates.

A similar example would be the hypothesis ' All deciduous trees are less than 200 feet in height '. Here again we would look at the trees which are more than 200 feet in height to see whether or not any of them are deciduous, rather than make records of the various deciduous trees less than 200 feet high.

In general, if the probability of an object being ϕ is x and the probability of it being ψ is y ($y \geqslant x$ since we assume that it is possible for all ϕ objects to be ψ) the distribution if there were no correlation would be

$$\begin{array}{llll} \text{(a)}\ \phi \cdot \psi & \text{(b)}\ \phi \cdot \sim\psi & \text{(c)}\ \sim\phi \cdot \psi & \text{(d)}\ \sim\phi \cdot \sim\psi \\ xy & x(1-y) & y(1-x) & (1-x)(1-y) \end{array}$$

and if the hypothesis were true

$$\begin{array}{llll} x & \text{——} & y-x & 1-y \end{array}$$

The first line of argument would be as follows. If there were in fact no correlation between being ϕ and being ψ, then the chances of a random collection of n ϕ objects all being ψ (i.e. all *a* instances and no *b* instances) would be $(y)^n$, the chances that a random collection of $\sim\psi$ objects will all be $\sim\phi$ (all *d* instances and no *b* instances) would be $(1-x)^n$. Thus if and only if $y < 1 - x$ will *a* instances provide better confirmation than *d* instances.

The second, and less cogent, argument gives the same condition. According to this the confirmatory power of different types of instance will depend on the relative expectations of finding such instances at all, according to whether there is no correlation or a complete correlation. Thus, if there is no correlation the probability of an *a* instance occurring is xy ; if the hypothesis is true the probability is increased to x, that is increased by a factor of $\frac{1}{y}$. The similar increase in the probability of *d* instances willl be by a factor of $\frac{1}{1-x}$. Thus the relative increase will be greater for *a* instances than *d* instances if and only if $y < 1 - x$.

We are now in a position to draw some conclusions. The first is that a necessary condition for *a* instances to provide better confirmation than *d* instances is that the probability of an object being ϕ shall be less than the probability of it being $\sim\psi$ — e.g. the probability of something being a raven less than the probability of it being not-black.[1] It is obvious that this condition is almost always satisfied in the hypotheses expressed in an ordinary language ; for almost always the subject and predicate classes will be small compared with the total universe of discourse. In situations where the condition does not hold we would almost certainly have simple predicate phrases to refer to the classes complementary to those of x and y so that we should in practice be more likely to formulate the hypothesis in the form ' All $\sim\psi$ objects are $\sim\phi$ ' e.g. ' all long-haired men are graduate students ' or ' all the trees over 200 feet high are coniferous '. So I would suggest that the paradoxes arise because we always tacitly presuppose that this condition holds. Hempel's concept of confirmation seems outrageous because he is considering the ideal situation in which our minds though well-stocked with concepts are *tabulae rasae* as far as facts are concerned, the sort of situation in which we know how to recognise ravens and black objects, but in which we do not know that the probability of finding a raven is very considerably less than the probability of finding an object (animal, vegetable, or mineral) which is not black.

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[1] This conclusion has already been derived as a theorem in the development of an axiomatised theory of confirmation by Dr Janina Hosiasson-Lindenbaum, *The Journal of Symbolic Logic*, 1940, **5**, 133-148

Secondly we might draw an additional argument against Watkins's position as actually stated by him. For in one of the passages about the raven hypothesis he says that statements about non-ravens cannot confirm it because they do not report tests of the hypothesis (Watkins, 1958, 351). This would suggest that in the exceptional cases, such as the case of the missing long-haired undergraduate or the deciduous trees, Watkins would have to say that observation of long-haired graduate students or coniferous trees more than 200 feet high would not count as reports of tests of the respective hypotheses and so would not count as confirmatory. And this, we have seen, would be wrong.

But further reflection shows that perhaps in one way Watkins is right. For the important feature of such arguments is not the mere occurrence of *a* instances (or, in the exceptional case, *d* instances) but their occurrence in conjunction with the absence of *b* instances. If we only knew that so many *a* instances occurred in a collection of objects otherwise unspecified, we should be forced to rely on what I called above the second and less cogent argument from the expected frequency of *a* instances in the total population. This line of argument would involve some complicated calculations and would be impossible unless we had accurate knowledge of the actual probabilities of an object being ϕ, or of being ψ. In fact the argument on which we surely normally rely starts from the fact that unless there is a connection between being ϕ and being ψ, it is extremely unlikely that we should get a collection of n ϕ objects all of which are also ψ. In the exceptional case we would argue similarly from the improbability of getting $n \sim\psi$ objects all of which are also $\sim\phi$.

One puzzle remains—the status of *c* instances. Why are *c* instances not confirmatory ? For surely one could argue in a similar way from the improbability of getting in a collection of objects so many *c* instances and no *b* instances. The answer is two-fold. Firstly it will always be the case that either *a* or *d* instances will provide better evidence than *c* instances. Secondly that in practice we are very unlikely to consider them ; for it would involve searching for objects that were either *b* or *c* instances, i.e. either $\phi \cdot \sim\psi$ or $\sim\phi \cdot \psi$, that is, for instance, looking for objects which were either non-black ravens or black non-ravens. It would always be easier to search for ϕ objects, whether ψ or $\sim\psi$ (*a* and *b* instances) or for $\sim\psi$ objects, whether ϕ or $\sim\phi$ (*b* and *d* instances).

What I have been trying to do in the second half of this note is to explain *why* we normally regard *a* instances as alone providing confirmation. The problem is here that of elucidating our usual methods of argument in cases where we speak of evidence confirming a hypothesis, cases in which we already assume some prior knowledge ; it is not the problem of providing a better method of argument based on a new formally defined concept of confirmation and applying this method to hypotheses considered in complete isolation. My conclusion was firstly that in such cases our method of argument is from the improbability of getting so many *a* instances and no *b* instances if there were no connection, and secondly that in such arguments we presuppose that the probability of an object being ϕ is much less than the probability of it being $\sim\psi$—a presupposition which is almost always justified.

H. G. ALEXANDER

REVIEWS

KOYRÉ ON THE HISTORY OF COSMOLOGY

This is a straightforward story, lucidly told.[1] It is the story of the development of the finite universe of the middle ages into an infinite one. We see how the finite universe slowly disintegrates and the infinite universe of Newton ultimately establishes itself.

Koyré's straightforward story is not a story without a moral. In the introduction to this volume Koyré refers to the views which animate his presentation. His basic idea is that the rise of modern science is bound up with a change of world-view, with a change, that is, of metaphysical outlook. Now this is surely a thesis which flatly contradicts the central thesis of positivism, or anti-metaphysics, or empiricism. (In Koyré's terminology ' empiricism ' signifies the view that scientific assertions are proved by experience.) But Koyré presents his thesis with no direct reference to this as his arch-enemy. Positivism, Koyré asserts, is a very ancient doctrine; he sees in it an enemy who is an old acquaintance too. Perhaps it is his familiarity with this old enemy which leads Koyré merely to point an occasional shaft in its direction, as when he expresses his feelings by adding the word ' disastrous ' to a remark concerning the present divorce between science and philosophy. Yet his main thesis is just this: ' The conception of the infinity of the universe is, of course, a purely metaphysical doctrine that may well—as it did—form a basis for empirical science; it can never be based on empiricism ' (p. 58). This is the moral of the present volume—even though it is stated explicitly only when it serves as a part of a specific historical explanation. And the power of the present volume lies in the obvious fact, that in spite of the author's conscious attempt to avoid side issues as much as possible, he is *forced*, time and again, to discuss metaphysical and even theological arguments, without which his story could not be presented adequately or even intelligibly.

I do not see how a positivist can possibly answer Koyré's historical argument convincingly. Indeed, I think that most positivists who have studied the history of Renaissance physics—including Duhem—have had to concede something, however grudgingly, to the kind of view which Koyré adopts. Yet Koyré's work shows how very much they have to concede. Of course, one can still admit refuting instances, and declare that the refuted theory holds for all other cases. In science this procedure is considered very unsatisfactory, and a theory which is rendered too shifting and *ad hoc* by this procedure will sooner or later be rejected. In philosophy it is customary to dismiss the refuting instance with an old and lame excuse, as Stuart Hampshire did quite recently, claiming that the metaphysical basis of modern science is merely a peculiarity of its infancy. The metaphysical systems of the Renaissance, Hampshire admits, are ' evidently designed to prescribe the outlines of a new physical science '. Yet they were concocted only as an antidote to Aristotle; in our own day ' the construction of metaphysical systems of any kind has come to seem finally useless and impossible '. Again, ' we no longer have any need for armchair programmes of science '.[1] What admittedly worked in the past is declared to be impossible now. But Hampshire's argument is very unconvincing (if for no other reason than that we *may* need an antidote to the antidote to Aristotle). His argument was already stated at least a century ago, for it was reported and answered by Maxwell. ' It has been asserted ' Maxwell tells us,[2] ' that metaphysical speculation is a thing of the past and that physical science has extirpated it. The discussion . . ., however, does not appear to be in danger of coming to an end in our time, and the exercise of speculation continues as fascinating to every fresh mind as it was in the days of Thales.' In other words, science is always in its infancy; in each period we start a *new* discussion concerning *new* problems, and the starting point is speculative or metaphysical. Thus, the ' useless and impossible ' discussion is still going strong.

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[1] A. Koyré, *From the Closed World to the Infinite Universe*, Johns Hopkins Press, Baltimore (London: Oxford University Press). Pp. x + 304. 40s.

I have tried to present and defend the methodological views implicit in Koyré's book, believing that I may thus be able to show the topical interest of what is primarily a historical volume. I believe that even the most convinced positivist, if he will be critical enough to accept the challenge and read the present volume carefully, will find doubts uneasily stirring in his mind. Having emphasised its philosophical significance I shall now offer some further, mainly historical, additional remarks concerning Koyré's subject-matter. I wish to emphasise, however, that these remarks are merely intended as marginal notes to a fascinating book. The main point of the general remarks is more to give the reader a general idea of the background than to criticise Koyré's brilliant historical synthesis.

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[1] Stuart Hampshire, *Spinoza*, Penguin edition, 1951, Chapter VI, pp. 211-212. Cf. Duhem, *The Aim and Structure of Physical Theory*, Princeton, 1954, Part II, Chapter 2. Hampshire presents this view tentatively, and even reports later some vague objections to it. But he ultimately commits himself to this view (p. 226). One need hardly say that this view contradicts the linguistic form of positivism (according to which metaphysical ' utterances ' can never have any meaning). Yet after a sensible presentation of Spinoza's metaphysics and after admitting its relevance to physics, Hampshire remains true to linguistic positivism, considering metaphysics as meaningless and divorced from science (at least from now on).

[2] James Clerk Maxwell, *Scientific Papers*, Cambridge, 1890, Vol. II, p. 225. For Maxwell's marvellous ' armchair program ' see his letter to Faraday of 1857 published in Rollo Appleyard's *A Tribute to Faraday*, 1931, p. 111-13. Faraday's reply to this letter is quoted below, p. 238, n. 2. For his despair of the programme see op. cit., p. 125 and introduction to his *Matter and Motion*.

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1. Koyré's discussion of the priority concerning the view of the infinite universe may be correct. But it is not supported by any argument against Jammer's attribution of the idea to Rabbi Hasdi Crescas (*Concepts of Space*, pp. 79–80). This is a point at issue, since Jammer contends—rightly or wrongly—that Crescas influenced Telesio who, in his turn, influenced Giordano Bruno to whom Koyré attributes priority (p. 39). But it is preferable not to discuss priority of the general idea—which is ancient anyhow—but the question : who raised the *problem* by criticising accepted views ? From this angle I tend to believe that Crescas's criticism of Aristotle was of paramount importance, while, no doubt, his positive theory (being pre-Copernican) seems less imaginative and revolutionary than that of Bruno.

2. Koyré suggests (p. 56) that Gilbert hesitated over whether to accept Copernicanism. The basis for Koyré's suggestion is the fact that, while arguing for the existence of the earth's diurnal motion (or rotation round its axis) Gilbert ' consistently avoids saying ' (p. 286) explicitly whether or not he admits the earth's annual motion (or rotation round the sun). Koyré asserts his view diffidently, and even mentions a weaker alternative to it, namely, that dealing with terrestrial phenomena (magnetism) Gilbert was uninterested in the problem of the annual motion of the earth. But both these hypotheses are untenable.

Gilbert does not consider magnetism to be a terrestrial phenomenon but rather a cosmic feature which he identifies with form (in the Platonic sense), with light (in the wider neo-Platonic sense), and with force (which is his own new metaphysical notion). Thus he considers magnetism to be the cause of all celestial action.[1]

Moreover, Gilbert does commit himself to Copernicanism, even though he deliberately avoids speaking explicitly about the annual motion of the earth. This is clearly seen in the following passage (from Chapter III, Book IV, of *On Magnets*): ' III. *That the Earth moves circularly.* I pass over the reasons (*sic*) for the Earth's remaining motions, for at present the only question is concerning its diurnal motion.' In another passage (ibid., Chapter IV), not only the Copernican theory but also the Copernican Sun-worship are manifest: ' the Earth is moved by her own primary form and natural desire, for the conservation, perfection, and ordering of its parts, towards things more excellent: and this is more likely than that the fixed stars, those luminous globes, as well as the Wanderers, and the most glorious and divine Sun . . . should circulate aimlessly round the Earth. . . .' Or, in other words, the theory of the Earth's diurnal motion is more plausible since it follows from Copernicanism. (By the way, he speaks very highly of Copernicus as the reviver of astronomy.)

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[1] For all this see last pages of *On Magnets.*

Furthermore, Gilbert speaks of the sun as the chief cause of motion, and he explains the different length of the different planetary years by assuming a fixed proportion between each planet's year and its distance from the sun. These two theories make Copernicanism an inescapable conclusion even though in this place his evasion is most peculiar. (As Professor Koyré kindly pointed out to me this law of linear relation between the planetary year and the planetary distance was four years previously published by Kepler in his *Mysterium Cosmographicum* ; he had thought of it before he hit on his famous ' third law ' and soon rejected it as inadequate. Rough linearity was already indicated by Copernicus.)

Ultimately, the only point of Gilbert's theory of gravity (which is, again, an elaboration of Copernicus's view) is that it enables the earth to move in space (and it is for this reason that Galileo accepted it from Gilbert). The theory is, briefly, that gravitation is merely the tendency of the parts to unite with the whole, so that the whole itself has no weight and is free to move, not only round its axis but also in space. He concludes his chapter on gravity (Book VI Chapter 5) thus : ' Therefore weight neither hinders the diurnal revolution, nor influences either direction or continuance in position.'

These are my reasons for rejecting the tentative suggestions which Koyré does not propound with much conviction anyway. Koyré has raised here a fascinating problem which was hardly noticed before.[1] It is indeed a strange fact that careful reading of Gilbert convinced even a judicious scholar like S. P. Thompson,[2] that he definitely asserted something which, on closer inspection, will be found to be at most only an implication or even merely an allusion. Although, like Thompson I regard Gilbert as a Copernican, I have no solution to Koyré's problem about why Gilbert very carefully refrains from a frank assertion of the existence of the earth's annual motion. (It should be remembered that Gilbert had no religious scruples here: the greater heresy was to admit the earth's diurnal motion, as Joshua attributed diurnal motion to the sun.)

[1] Dreyer, to whom Koyré fully refers, merely asserts that Gilbert was a follower of Brahe ; which, though palpably false and superficial, might, perhaps, pass as a kind of indication of the present problem. But Dreyer was the first to notice Gilbert's evasion.

[2] S. P. Thompson, *Notes on De Magnete*, privately printed, London, 1901, especially p. 62. See also his *Gilbert of Colchester*, London 1901, p. 31, where he speaks of Gilbert's ' frank acceptance of the astronomical doctrines of Copernicus '. This assertion is refuted by Koyré.

However, it is quite clear that Gilbert's work is designed to assist the establishment of Copernicanism. There may have been two reasons for his embarking on his magnetic research. The one is that the magnetic theory accepted at his time (that of Paracelsus and Cardan) assumed magnetism to be caused by some celestial bodies, which Gilbert tried to test, evidently with an eye to its possible relevance to the astronomical controversy, and which he later refuted. The other is this. The main obstacle to Copernicanism was Aristotle's theory of natural motion, which asserted that natural motion is either circular or in a straight line towards the centre. Since Copernicus deprived the earth of its central position it was necessary to find independent arguments against Aristotle's theories of motion and of gravity—a problem dealt with mainly by Gilbert and by Galileo. Indeed Gilbert asserts that he considered the natural motion of the magnetic needle (circulating towards a fixed direction) as a refutation of Aristotle.

So much for Gilbert's initial purpose. As to his main achievement, he believed that his magnetic theory gave new arguments in support of the earth's diurnal rotation, and this may well be why he did not wish to assert more. This idea gains slight support from a work of Barlowe, a friend of Gilbert who defended his views on magnets though not on astronomy:[1]

> But concerning his (Gilbert's) Sixth Booke entreating the motion of the earth, I thinke there is no man liuing farther from beleeuing it, than my selfe, being nothing at all perswaded thereunto, by the reasons of other men, which he alledgeth, and as little or lesse (if it were possible) by those his inventions, endeauouring to proue the motion of the earth by the earthes *Magneticall* force and vertue.

The 'reasons of other men which' Gilbert 'alledgeth' are mainly those of Copernicus, of course ; though Barlowe is vague on this question. It is remarkable that even Barlowe, Gilbert's only disciple, was unwilling to admit Gilbert's strongest astronomical point, and this may partly explain Gilbert's reluctance to go further into the matter. I do not know how much one's pupils' dogmatism may render one reticent as to one's revolutionary views, but it seems that these things do happen. In a reply to an open-minded letter of Maxwell, Faraday admitted[2] that he would not express in public certain revolutionary ideas of his. Gilbert was even less fortunate than Faraday, of course, the scientific tradition not having been developed as yet. However, I do not know how my conjecture can be checked, and I consider Koyré's problem still open.

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[1] W. Barlowe, *Magneticall Advertisements*, London, 1616, 2nd edn. 1618, preface to reader.

[2] H. Brence-Jones, *Life and Letters of Faraday*, London, 1870, Vol. II, p. 392: 'The idea that we may possibly have to connect *repulsion* with the lines of gravitation-force (which is going far beyond anything my mind would venture on at present, except in private cogitation) shows how far we *may* depart from' Newton's views on gravitation. (See note 2, p. 235, above for the reference of Maxwell's letter.)

3. Gilbert's further contribution towards Copernicanism is the new solution he suggested towards the problem of infinity. Koyré does not discuss the views of infinity in connection with Euclidean geometry (which is a pity), though he discusses this aspect in his *Galilean Studies*. For it is clear[1] that one of the main reasons for the acceptance of infinity is the Renaissance acceptance of the geometrical outlook, or, as Bruno calls it, Pythagoreanism. This is in fact Plato's revised version of Pythagoreanism (intended to solve the problem of the irrationals), in which geometry, or figure, takes the place of arithmetics, or number, and proportion takes the place of ratio. (This is admirably discussed in Popper's works[2]—for the first time, I think.) The interest of Kepler, Gilbert, and Galileo, in genuinely geometrical models (i.e. ones which contain proportions) is evidence, if evidence is necessary, for their Platonism; Copernicus's Platonism (or rather neo-Platonism) is expressed in the view that the sun symbolises the Idea of the Good and should therefore be the centre of the universe. (See Burtt's classical *The Metaphysical Foundations of Modern Physical Science*, London, 1925.) Now Euclidean geometry assumes an infinite universe, but the infinity of the universe is incompatible with the idea of a centre to the universe. This is why Copernicus and Kepler are finitists, while Bruno and Gilbert are infinitists. Gilbert's solution to the problem of centricity in an infinite universe is this: the sun is not in the central spatial position, but central in another way, being the primary mover, or the cause of motion. This 'magnetic' (i.e. dynamic) theory contradicts Plato's idea (in the *Timaeus*) that all (inanimated) motion is caused by impact or push, which Gilbert quotes disapprovingly. Thus Gilbert's solution is not quite acceptable to a full-fledged Platonist. (Indeed, Plato was a finitist; yet Euclid who carried out a part of Plato's geometrical programme, was forced into a different position.)

However, Gilbert was the person who made the Copernicans conscious of the fact that their acceptance of Euclidean geometry introduces problems which demand a solution. It is perhaps a pity that Koyré glosses over

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[1] Cf. 'All the movements of the loadstone are in accord with the geometry and form of the Earth and are strictly controlled thereby, as will later be proved by experiments and diagrams.' Or 'Hence the magnetic nature is proper to the Earth and is implanted in all its real parts according to a primal and admirable proportion.' (Pp. 67 and 105 in Mottaley's translation of *On Magnets*.)

[2] See K. R. Popper, *The Open Society*, 3rd edn., London, 1957, Vol. I, Chapter 6, n. 9 and *Addendum*, as well as his 'The Nature of Philosophical Problems', this *Journal*, 1952, 3, pp. 134-154. As to my remarks below concerning Euclid, see there pp. 147-148 and 152.

Gilbert's geometrism and dynamism, accepting the widespread myth of Gilbert's animism. This myth is partly due to the animism of the ancient Pythagorean school, partly due to mechanist prejudices (mechanism being the theory that all acceleration is caused by push, which Galileo, Descartes, and Boyle, made the authoritative view then), partly due to the later tendency to emphasise the difference between Gilbert and Newton, and partly due to a still later confusion caused by a failure to realise that by endowing magnets with souls Gilbert merely endowed them with (force and) harmony, following the Pythagorean-Platonist theory which viewed the soul (as self moving and) as harmony. The viewing of the world as harmonious was shared by Oersted (the author of the wonderful *The Soul of Nature*, London, 1848) and Einstein, among other modern philosophers. The view of the soul as a harmony is also not quite an outdated view, be it true or false. Admittedly Gilbert is more anthropomorphist than, say, Einstein, but this is due to the general tendency towards ever increasing abstractness.

4. Koyré asserts that Kepler rejected the view of the infinite universe on empiricist grounds and that Kepler even claimed that the idea of infinity is meaningless. It seems to me, however, that even if Koyré interprets correctly the texts he relies on, he takes them much more seriously than Kepler had intended, considering that Koyré himself does not claim that Kepler was an empiricist. Kepler's position is quite clear. ' My opinion that there are unseen comets in the sky is disputed by many ' he writes.[1] ' One asks how could I know this. But I do not say that I know it. . . . You believe that one should see them if they exist. I deny this.' And in many places he praises, like Galileo, the method of hypothesis, as the opposite of the theory which bases scientific views on observations. It seems to me that Koyré slightly underrates Kepler as a methodologist, although I admit that there is a kernel of truth in his exposition, and even an important one which enables us to accept and even strengthen his presentation.

Kepler believed (like Galileo) that the world is knowable and that therefore false hypotheses should be rather easily refutable within a short time so that truth will soon be established.

This optimistic theory of the 'nearness' of the truth[2] lends some force to Kepler's central argument against infinity. The argument, which Koyré discusses in detail, is very roughly this. If the world is infinite then the two following propositions must hold. (*a*) The stars must be well ordered, say, like atoms in an infinite crystal. (*b*) The world looks pretty much the same from any position. And Kepler shows that the conjunction of these two statements is refuted, because from certain stars other stars appear about three times larger than the apparent size of the sun as viewed from the earth. Kepler's argument is beautiful and most imaginative, yet the connection between the infinity of the universe and the conjunction of the two propositions (*a*) and (*b*) which he tried to refute is not clear unless one remembers his optimist view of the world as being sufficiently simple. (Incidentally the second proposition, that the world must look pretty much the same from any position, is strikingly reminiscent of the modern cosmological principle. The difference between Kepler and the moderns is this. Kepler's physics is mainly geometry and he therefore prescribes similarity of geometrical initial conditions, i.e. position and magnitude, while the modern cosmologists postulate a similarity of initial conditions relevant to modern theories.)

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[1] Carola Baumgarten, *Johannes Kepler : Life and Letters*, London, 1952, p. 78.

[2] For this theory see K. R. Popper, ' Die öffentliche Meinung im Lichte der Grundsätze des Liberalismus ', *Ordo*, **8**, 1956.

5. Koyré is puzzled by Newton's use of the variability of gravitational forces to support his refusal to consider gravity as an essential property (p. 175). An essential property is constant. Thus, since mass is constant, inertia which depends on mass only is essential, while gravitation cannot be essential since it is variable, as it depends on distance which constantly changes. Koyré, however, thinks that from Newton's own point of view gravitation is just as constant as inertia. 'The *weight* of a body', he agrees, is indeed variable, 'but the attractive force of the earth . . . is constant, and, just as in the case of inertia, proportional to its mass, and it is as such that it appears in the famous inverse square formula of universal gravitation' he asserts. I am afraid I am unable to follow this argument unless it is this: Newton's *gravitational mass* is constant, while merely the intensity of its effect—the *gravitational force*—decreases as the effect of a source of light of a constant intensity decreases with the increase of the distance from it. This is the argument of Roger Cotes (Preface to second edition of *Principia*). But Cotes's argument is evasive since it is not the propensity to gravitate but the actual gravitational force which is under discussion, and here Cotes's analogy breaks down, since the light particle is a *thing* while the force is the *action* of one thing on another. As Professor Popper puts it, Newton could not view gravitation as essential since it is *relational*, while an essential property can belong only to the one substance whose essence it determines. The variability of gravitation proves its relational character.

6. One of the most interesting aspects of the book is the discussion of the relations between natural philosophy and rational theology. Here the discussion of the physico-theological (teleological), proof of the existence of God. The physico-theological 'proof' is this: the existing order of the universe is an argument in favour of the theory that the universe was ordered by a divine architect. Yet the historical significance of the 'proof' lies in the fact that it was inverted. The theory of a divine

orderer was used to support or undermine different theories concerning the order of the universe itself. For the divine architect had a divine purpose in ordering his world, and the (purposeful) order is such that becomes the architect who is so truly divine. This inversion of the argument not only explains why the physico-theological proof was indeed used, by Bentley and others, as physico-teleological—to use Koyré's label—but it explains indeed, as Koyré shows in great detail, the penetration of theology into scientific discussions.

The general view in the mid-seventeenth century was that theology and philosophy should be kept apart. This view was accepted on the authority of Galileo, Bacon, and Boyle. Boyle (who introduced the label 'physico-theological') was one of the most ardent separators of religion and philosophy. He held an all-embracing metaphysical dualism with reason, physico-theology, philosophy, and justice on one side, and on the other, emotion, religion, fine arts, and mercy. He concluded from this that the religious arguments from miracles have no rational value whatever, and have merely a sentimental appeal. He saw in the physico-theology (or rational theology, or natural theology) the basis of natural religion (or rational religion) and philosophising—experimenting included—as the ritual of that religion. Physico-theology served as an encouragement for philosophic activity.[1] It also served as a safeguard to assure peaceful co-existence between philosophy and religion (by excluding scientific materialism which might incite hostility towards religion and also by prohibiting religious disputes in philosophical circles).

But Boyle's Cartesian universe which seemed at first so well ordered had certain defects. The watch demanded not only a watch maker (to use Boyle's metaphor) but also a constant rewinding and mending (to use Koyré's metaphor). The argument about whether it is becoming for the architect to make a watch which is in need of rewinding put the problem of conservation of energy in the centre of the controversy. This controversy probably started with Bentley's *Boyle Lectures*, which were intended as a reassessment of Boyle's philosophy, and which were never published in their original form, because of the famous correspondence between Bentley and Newton, which forced Newton, and later Bentley, to change their views. Bentley forced Newton to think about the theological implication of his own theory, especially in the light of the problem of the validity or invalidity of the mechanical philosophy. For Bentley's starting point in his Boyle's lectures was this mechanical philosophy, and he wanted Newton to corroborate both mechanism and its (alleged) theological implication. Challenged, Newton worked at his problem, but he ultimately gave up hope of finding a solution. In order to understand the *instantaneous action at a distance* of the stars upon one another, he called in the omnipresence of God. (I owe this point to Professor Popper.) Thus God was introduced as the frame of the world machine, if not just a cog in it, much to Leibniz's dismay. Attacking Newton's heresy (while referring to Boyle's piety and to Boyle's mechanism), he alleged that it is unbecoming for God to create a watch which cannot maintain itself. This argument gave rise to the Leibniz-Clarke *Correspondence*, which, as, Koyré shows, brought the problem of the conservation of energy (or *vis viva*) into the foreground. The surprising fact was that Newton's theory turned out to conform to the Leibnizian law of conservation of energy, which, in its turn, was factually corroborated. The watch needed no rewinding after all. Physico-theology could be relegated to the wings, if not ignored altogether.

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[1] Boyle's so-called 'theological' writings, from his first publication (*Seraphick Love*) onwards, were not theological, but outspokenly propaganda for the new experimental philosophy.

The story of Laplace's answer to Napoleon ' Sir, I do not need this hypothesis ' (of the existence of God) is very famous. Koyré has succeeded in putting it in its historical perspective and in giving it a new interest by interpreting it literally rather than as a sarcastic remark. It marked the end of an interesting intellectual co-operation between theology and philosophy, and the starting point of the nineteenth-century scientific materialism, a movement which dismissed this previous co-operation with theology as a minor incident. It was soon entirely forgotten, to be rediscovered only in our own time.

7. Physico-theology is only one of the historical roots of this co-operation. The other was the discussion between More and Descartes. Descartes, the Copernican, refused to ascribe infinity to space (for reasons discussed above, under point 2). He declared that God and only God is truly infinite. More, the infinitist, overcame this theological difficulty when he identified God and space, thus anticipating Newton's theory of space as God's sensorium. I think both suggestions, Descartes's as well as More's, are rather evasive. Descartes's idea of an ' indefinite ' universe is vague, and is used as an evasion of a genuine problem. More's criticism of Descartes, based on the interpretation of ' infinite ' as ' non-finite ', seems to me unanswerable. If Descartes's ' indefinite ' implies merely that the size of the universe is unknowable, then the question of the truth or falsity of Euclidean geometry is undecidable, which is counter to Descartes's view of Euclidean geometry. If the universe is non-finite, but of a different category of non-finitude than the one to which God's non-finitude belongs, then Descartes had committed himself to the view that the sun is not in the centre of the universe while, in fact, he did not wish to commit himself on this issue. (As Koyré argues, he held his tongue in his cheek, believing in Copernicanism while pretending to conform to Catholicism.) However, his excuse for his evasion—his refusal to ascribe infinity to anything but God—had to be answered, and was answered by More's identification of God and space.

I do not find the identification very impressive, in spite of all the Cabbalistic arguments which More marshalled in defence of this view. Yet even this unsatisfactory debate between Descartes and More was pregnant with interesting ideas which were pursued in the dispute between Newton and Leibniz mentioned above.

8. The end of the relationship between natural philosophy and natural theology was primarily due to the success of Newton's natural philosophy, and also to the decline of theology in general, as Koyré points out. The decline in theology was in turn due largely to the wave of Biblical criticism and to criticism of the pretensions of theology which had already started in the seventeenth century. As Mr J. W. N. Watkins kindly pointed out to me a good deal of this Biblical criticism was historically based on a theory of the ' nearness ' of (religious) truth analogous to the theory of the ' nearness ' of (natural) truth mentioned above, under point 4. The most impressive criticism of theology was perhaps Kant's refutation of the traditional proofs of the existence of God. Koyré rightly mentions (p. 180) Kant's appreciation of the physico-theological proof, but it should be added, perhaps, that Kant appreciated it only as an argument, not as a logical proof. Kant himself was satisfied with a deity whose existence is undemonstrable, but for others this was a dethronement of God, or, as Heine put it rather poetically, the death of God.

Kant's reintroduction of God in his system of ethics, is a much discussed point. But he also introduced God, in a fashion, already in his philosophy of science. This seems to me to be his Boylean approach, common to many thinkers of the period of self-improvement or Enlightenment, that is to say, the view of philosophising as a natural worship of God, or as a moral duty. Natural philosophy was an active ' intellectual love of God ' in Spinoza's idiom. Today ' secular ' ethics had freed itself of theology, and so has philosophising. Even Einstein, who considers the ' cosmic religious feelings ' as essential for scientific research, refuses to take any interest in any personal deity whatsoever.[1] The intellectual interest in God seems to be a thing of the past.

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I have made the above notes in an attempt to give to the reader a general idea of the book. I have left out many interesting points in an attempt at an outline of some general themes in the work. The apparent form of the work is that of a presentation of various views of various thinkers concerning the question at hand, the question of the size of the universe. Yet, of course, two or three themes become rather outstanding in the discussion of that question. I have mentioned Copernicanism and geometrism and the problems they involve, the mechanical philosophy and its amendments which call in theological considerations. All these points are discussed by Koyré with great lucidity. The greatness of critical speculative thinking is in working out positions in detail, and Koyré does justice to his heroes by showing, in each case, how their views on a specific question (is the world finite ?) are linked with their other views ; whether they present together a coherent picture of the world ; and how these coherent world-pictures were criticised.

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[1] A. Einstein, *The World As I See It*, ' Science and Religion '; also *Out Of My Late Years*, ' Science and Religion '.

I wish to repeat that my remarks are merely intended as marginal notes to a fascinating book. It is much easier to sum up all one's comments and criticism of a good book than all that one wishes to say in its favour, or even all that one might say about the new ground gained in it. I should only mention that, among other merits, its presentation of Descartes's natural philosophy, of Kepler's arguments, of Raphson's and Bentley's views, and of the development of the views after Newton, are, to my knowledge, entirely new, and truly fascinating. The work is a model of clarity, precision, and documentation. In the best French tradition it makes very easy reading yet at no sacrifice of profundity. With very few authors Koyré might, in good conscience, repeat Kepler's assertions [1]:

> 'I have therefore wished to remind the reader that everyone likes his own way best, that most people get heated in their discussions and that, in my opinion, humour is by far the best seasoning of any debate. Others, in philosophising, try to impress people with too much grave solemnity but very often give a ridiculous impression this way quite against their will. I believe that I am made for mitigating the troubles and bothers of research work by expressing good humour in my style.'

J. Agassi

The Philosophy of Science. By P. Henry van Laer, in collaboration with Henry J. Koren.
Editions E. Nauwelaerts, Louvain, 1956. Pp. 159. $3.75.

In this book and its projected successor, Dr van Laer, who is professor of philosophy at Leyden, has set himself a very ambitious programme. In the present volume he wishes to deal with the problems common to all the sciences; in the second, to characterise the several groups of sciences. By 'science' he does not mean the natural sciences alone; he includes not only the social and psychological sciences but also philosophy and theology. He lists the common characteristics of these studies: logical order, a definite field, interest in universal statements, and so on. He derives a list of their 'constitutive elements or aspects' (p. 14). They all use abstraction; their statements have a character of necessity; they have each their characteristic area of study and definite point of view, their starting-point and presuppositions, their proper methods; they all have a structure of hypothesis and theory, and depend on the logic of proof. This analysis constitutes the author's programme, and he proceeds to devote a chapter to each point. He wishes to give a general account of the terrain, before drawing a detailed map of the region of systematised knowledge.

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[1] Carola Baumgarten, op. cit., p. 83.

Such an attempt was worth making, for the time is ripe for comparison of the methods of different fields of study, in relation to the different problems to which they are relevant. The programme, therefore, is a promising one. Unfortunately the working-out is less satisfactory, especially as far as natural science is concerned. Dr van Laer has clearly a good grasp of physics and of traditional logic, and a wide range of interests; but he passes altogether too lightly over the special problems of the natural sciences. In these sciences we are faced, as Mr Kneale put it in his *Probability and Induction*, with 'untransparent necessitations'; the reasons why things should be thus and not otherwise are opaque to us. Dr van Laer assumes too easily that we have insight into the nature of things, and this leads him to underestimate the difficulties involved in assessing the status of scientific laws and theories. He thinks that we know by direct insight that nature is deterministic and must therefore be regular in operation. Such an easy solution of the question of induction—which according to Dr van Laer is a problem only for empiricists, not for realists—is the more surprising in that Dr van Laer has had experience of physical research, having worked with Professor Keesom on the properties of superconductors at low temperatures.

The book invites comparison with another recently-issued work, written within much the same tradition, which also in its way aims at a map of knowledge: Jacques Maritain's *Degrees of Knowledge*. Maritain makes less use of formal logic, and his book, which is much longer, is in some ways more diffuse; yet it seems to the reviewer to give a more delicate and more accurate picture of the range of human knowledge. Again, Maritain is no physicist, but he shows a surer touch in his treatment of the physical sciences and the revisions they require of the traditional treatment of abstraction. Neither work takes adequate account of English and American work in the last forty years on the logic of science.

The English of this edition is in general clear and readable, but a number of oddities need to be corrected: *crystallic* for *crystalline* (p. 45), *realistic* for *realist* (p. 131), *genial* for *of genius* (p. 120), *facilities* for *faculties* (p. 149), and the terms *ideal* and *experiential* applied to sciences; while John Stuart Mill is made curiously unfamiliar by the omission of his first name.

E. F. Caldin

Philosophy of Science. By Gustav Bergmann.
University of Wisconsin Press, Madison, 1957. Pp. xiv + 181. $4.75

In this book Professor Bergmann presents us with the first part of a more lengthy enquiry which he is making into the foundations of psychology. The book is, as he explains, entirely introductory in character, and it is unfair to criticise it as though it were the complete and finished product. Here he sets out what he regards as the necessary philosophical instruments for his whole enquiry, which is to be completed in three parts. Furthermore, his attitude towards these philosophical instruments is candidly utilitarian: both his choice of instruments and the precision with which he presents them are to be justified in the light of their use in his subsequent discussions. The two succeeding parts of the work are going to comprise, firstly, a behaviouristic analysis of psychological concepts and, secondly, a history of the philosophy of psychology starting from the time of Galileo and Descartes.

Professor Bergmann's modesty about his aim in the present book somewhat disarms the critic. All one can say is that he sets out here certain fairly clear-cut distinctions in the philosophy of language and related fields, which he claims to be indispensable for his subsequent analysis of psychology and the philosophy which it has engendered. These are roughly speaking those which one associates with the phrase ' logical empiricism ', though Professor Bergmann is careful to reject the phenomenalism which has so commonly been associated with that point of view. To summarise:—There are descriptive or naming words and there are words which are not descriptive or naming ones; there are synthetic or substantial propositions, and others which are linguistic, analytic, or formal in character; proof is a matter of deduction and a theory is a group of laws deductively connected; all definitions are analytically true and arithmetical propositions are tautologies; celestial mechanics provides the paradigm of explanation—historically in fact, and also for the logician analysing the notion of explanation; nevertheless it may well be—and it will be interesting to see how Professor Bergmann follows up this clue in later volumes—that some historical laws of an irreducible kind may be unavoidable in psychology and other behaviour sciences. Certainly the conditions which would need to be fulfilled in order for there to be a successful ' reduction ' of psychology to physiology are extremely stringent, and one may well doubt (Professor Bergmann insists) whether they are ever capable of being fulfilled completely.

There is indeed a good deal of general interest and value in Professor Bergmann's book, especially in the detail of the arguments he employs. But certain criticisms are unavoidable: both as to the content and as to the style. Technicality is the price we pay for precision, and the fact that Professor Bergmann is writing largely for psychologists does not seem to me to excuse the looseness of his exposition. Perhaps the distinctions which he makes are going to pay off in the subsequent instalments, but it was a mistake to present them as being ' relatively uncontentious ', when so many of them are in fact subjects of acute philosophical debate. For instance, I do not believe that ' there is no argument in philosophy proper about the adequacy of the definitions of dispositions by if—then statements '; and it is certainly no longer true that Russell's theory of definite descriptions provides ' a circumscription that can in any context be substituted for a definite description without causing any trouble '. These are only two examples. Stylistically, too, Professor Bergmann has aimed at the colloquial and at the readily intelligible, but has achieved instead an extremely jerky and abrupt effect. All the same, I for one very much admire Professor Bergmann's courage in embarking on this rather difficult enterprise, and shall look forward keenly to seeing the further instalments which he promises us.

STEPHEN TOULMIN

Logic without Metaphysics, and other Essays in the Philosophy of Science.
By Ernest Nagel.
The Free Press, Glencoe, Illinois, 1956. Pp. xviii + 433. $6.00

IT is with some misgiving that one opens yet another collection of essays already published in various journals and reviews. This one makes a bulky volume, of which two-thirds is taken up by ten papers in the philosophy of science and related topics, variously dated between 1934 and 1954, and one-third consists of twenty reviews and review-articles, some of which can hardly be of other than ephemeral interest. When all this comes across the Atlantic at $6.00, one is tempted to be particularly demanding in one's assessment of the contents.

Perhaps, however, this is a jaundiced British view of academic publishing, and in this case, in spite of some inevitable repetition and outdated discussion, several of the articles contain matter of permanent interest. Professor Nagel is a clear and easy writer, and although he is in the forefront of the school of logical empiricism, he does not share the lack of interest in the history and sociology of philosophy which often characterises its native European members. Of particular historical interest is his account, written in 1936, of visits to the then centres of this philosophy in Europe : Cambridge, Vienna, Prague, Warsaw, and Lwow. The articles specially concerned with the philosophy of science contain discussions from the familiar empiricist point of view of such topics as the nature of logic and mathematics and their relation to empirical science, and the status of scientific theories. In an interesting article entitled ' Symbolism and Science ', theories are interpreted in a purely instrumental sense, as ' calculating and connecting ' devices, and the rôle in them of symbols and formal analogies is discussed.

In his Introduction, however, Nagel confesses himself dissatisfied with a purely instrumental view, but unable at present to find a satisfactory *via media* between the extreme standpoints on this issue, and he remarks that such very general problems cannot be solved wholesale, but only after careful analysis of particular examples. The hope that he will proceed to such analysis is in the main disappointed, for most of the subsequent articles remain on too general a level. This criticism does not apply however to an article on ' A Formalization of Functionalism ', for here Nagel gives an interesting and detailed comparison between a formalism describing functional or goal-directed systems as found in biology, and the data and concepts of the social sciences. This is an excellent example of the philosophy of science at work on a particular type of theory, and one hopes to see more from Professor Nagel in the same vein.

MARY B. HESSE

Meaning and Necessity, A Study in Semantics and Modal Logic.
By Rudolf Carnap.
Second edition, enlarged. The University of Chicago Press, Chicago, Illinois, 1956. Pp. x +258. 37s. 6d.

THE present edition contains the text of the first edition in its original form, and a supplement consisting of five previously published essays, which to a large extent were prompted by discussions and criticism of the author's semantical method. The subject-matter of the essays is reflected in their titles, which are as follows: (*a*) ' Empiricism, Semantics, and Ontology' , first published in the *Revue Internationale de philosophie*, 1950, **4**, 20-40; (*b*) ' Meaning Postulates ', *Philosophical Studies*, 1952, **3**, 65-73; (*c*) ' On Belief Sentences ' in *Philosophy and Analysis*, ed. M. Macdonald, Oxford, 1954; (*d*) ' Meaning and Synonymy in Natural Languages ', *Philosophical Studies*, 1955, **7**, 33-47; and (*e*) ' On Some Concepts of Pragmatics ', *Philosophical Studies*, 1955, **6**, 89-91. All these essays are related to various sections of the main body of the book. For the most part they clarify the author's position, and in some cases they modify his earlier views. In this respect, ' Empiricism, Semantics, and Ontology ' seems to be of particular interest as in its light many a student of semantics may have to revise his interpretation of Carnap's theories. It is, therefore, to this essay and its bearing on the foundations of semantics that the reviewer proposes to confine his observations.

The purpose of the essay, which is related to the discussion in sections 6 and 10 of the main body of the book, is to clarify the issue between those semanticists who admit abstract entities such as properties, relations, propositions, etc., and those who oppose the procedure. In order to understand more clearly the nature of the problem Carnap suggests that we should distinguish two kinds of question concerning the existence of entities: first, we have *internal questions*, i.e. questions of the existence of entities within a *linguistic framework*, secondly there may be *external questions* referring to the existence or reality of *the system of entities as a whole*. This distinction appears to constitute the substance of Carnap's argument against the nominalistic scruples in the Quine-Goodman style, and against the ' " Fido "—Fido ' criticism extended by Ryle. According to Carnap the acceptance of a kind of entities in the *internal sense* amounts to introducing (*a*) constants which are said to be names of the entities in question, (*b*) a general term or predicate permitting us to say of any particular entity belonging to the kind of entities under consideration that it does belong to this kind, and (*c*) a corresponding type of variables of which the entities are said to be the values, and for which the constants can be substituted. Now, the questions of the existence of any entities in the *external sense* are rejected by Carnap as pseudo-questions, and any thesis which gives expression to a belief in the reality of such entities is branded as a metaphysical pseudo-statement.

It would appear from what Carnap says in his essay that whenever he talks about introducing or accepting abstract entities, he no more believes in the reality of these entities than we believe in the reality of pieces of metal in our left foot whenever we complain that we have pins and needles in it. For, strictly speaking, the acceptance of entities in the internal sense—and it is only in this sense that Carnap seems to accept any entities at all—is in fact the acceptance of certain linguistic forms. If this is so then the reviewer sees no reason why it should not be conceded to Carnap that, contrary to the argument of some nominalists, the acceptance of linguistic forms involves us in no ontological commitments. After all, the ontology we embrace should not depend on the language we use but rather on what we assert with the aid of this language.

Let us, however, consider certain specific problems of semantics. In accordance with Carnap's intentions the semantical statement (1) ' The word " Chicago " designates a thing ' is to be interpreted as indicating that the object language which is presupposed by our semantics, is a *thing language*, i.e. a language within the framework of which we have (*a*) constant names, (*b*) a constant predicate which with any name as argument forms a true statement, and (*c*) variables for which names can be substituted. Moreover, (1) is to be regarded as analytic as it follows from the statement (2) ' The word " Chicago " designates Chicago ', which is supposed to be analytic in virtue of the general principle that any expression of the form ' " . . . " designates . . . ' is an analytic statement, provided the term " . . . " is a constant in an accepted framework (p. 217). In this connection one might remark that this is far from being a self-evident interpretation of the expression ' designates a thing '. And as regards the general principle it seems to be sufficient to put the word ' or ' in the place of the dots in order to undermine its alleged validity.

As far as the reviewer can judge, the word ' designates ' is quite commonly used in a somewhat different sense. One way of characterising this sense informally would be to postulate the truth of at least the following statements: (3) for all x, y, and z,—if x designates y and x designates z then y is the same thing as z, (4) for all x, y, and z,—if x designates z and y designates z then the corresponding statement of the form ' a is the same thing as b ' with x and y as the arguments is true, and (5) for all x and y, —if x designates y then y is real. Let us make it clear at once that the concept of reality which is presupposed in (5) is, to use Carnap's terminology, an ' empirical, scientific, non-metaphysical ' concept. Under such an interpretation statements (1) and (2) appear to be empirical statements. If it is empirically or factually true that the word ' Chicago ' designates something then it seems to be legitimate to ask whether every expression of our language has the same property or not. In particular it seems to be legitimate to ask whether expressions like ' Pegasus, is large ', ' Chicago is large ', have any designata or not. The critics of the ' " Fido "—Fido ' principle in semantics maintain that these expressions designate nothing, and that only names, except those which are fictitious, can designate. The term ' designates ' in the sense just explained, however, seems to have no place in Carnap's semantics, which in the light of the present essay turns out to be a theory of linguistic forms rather than a theory of relations that may obtain between the expressions of a language on the one hand and the things which constitute the world on the other.

The reviewer is left with the impression that Carnap is more successful in meeting the criticism based on the Quine-Goodman nominalistic doctrines, and that his appeal for tolerance in permitting the use of a wider variety of linguistic forms is likely to command considerable sympathy. In dealing with the critics of the ' " Fido "—Fido ' principle in semantics Carnap does not seem to be so successful. For the immunity which he secures against their objections, rests on what is likely to be regarded as an interpretative twist. If Carnap's semantical method has been misunderstood and misinterpreted by his critics owing to their failure to distinguish between the internal and the external questions of existence then it is not the critics who ought to be blamed for the confusion.

C. Lejewski

Probability and Scientific Inference. By G. Spencer Brown.
Longmans, Green and Co., London, 1957. Pp. ix + 154. 15s.

In fairness to the author it should be stated that he and the reviewer indulged in an ardent controversy on the first day of the Third London Symposium on Information Theory,[1] and readers of this review may therefore consider it appropriate to tone down some of the adverse comments.

In spite of its faults the book is entertaining, succinct, and provocative, and is written with some imagination. In fact the author could probably write good imaginative prose. As a writer on technical matters he is too fond of propounding paradoxes and leaving them unresolved for too long.

The main theme is that the foundations of existing theories of probability are self-contradictory, at any rate if the applications are included. The argument depends mainly on criticisms of the idea of a random sequence, and may be regarded as directed largely against the von Mises philosophy of probability, which is based on the idea of an infinite random sequence. It has in fact been recognised for many years that infinite sequences cannot have any direct operational significance ; nevertheless there is no mathematical contradiction in the idea [2] (although the author seems to imply that there is at the end of p. 56).

Modern theories of probability, which are based on such axioms as $P(A.B|C) = P(A|C)\,P(B|A.C)$, do not depend on the idea of infinite random sequences, but this idea is found to be mentally convenient in the applications. For example, if we have a machine that is supposed to produce 'perfectly' (equiprobably) random digits, then any finite sequence, S, produced by it, can be thought of as a finite segment of an infinite sequence, and such a segment may be called a 'finite random sequence'. Every finite sequence of digits, even say 3333333333, can occur in the output of a perfect randomiser. In practice if a sequence like 3333333333 is presented to us by itself, and not merely as a part of a sequence of about 10,000,000,000 digits, then we tend to believe that it did not come from a perfect randomiser by means of a place selection. (A place selection is one that is made in ignorance of the digits that will be obtained.) The reason for this belief is that the probability of obtaining this sequence is much higher given various alternative hypotheses.

It should be well known that the objectivity of orthodox statistics is obtained only at the expense of ignoring information. (See, for example, the reviewer's book,[3] p. 102.) Often the information ignored includes an exact statement of a choice of random numbers.[4] It is possible for the result of an experiment to be significant and yet most of the significance may belong to an unlucky choice of random numbers. It is partly for this reason that the practical statistician requires to use his judgment. For reasons of propaganda this necessity for judgment is ignored in a great many textbooks on statistics and in science in general : the writers are too anxious to establish the glorious objectivity of science. By pretending that science is entirely objective they reveal their own lack of objectivity.

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[1] *Information Theory*, ed. Cherry, London, 1956, see pp. 8 to 18

[2] See, for example, p. 14 of the reference in footnote 1

[3] *Probability and the Weighing of Evidence*, London and New York, 1950

[4] See, for example, *Journal of the Inst. of Actuaries*, 1956, **82,** 255

A long sequence of digits obtained from a good randomiser is bound to contain some very peculiar short sequences. A statistician who finds at the end of his experiment that he has used a peculiar subsequence may be worried about it. For this reason he may decide to avoid the use of peculiar subsequences. But then, as Spencer Brown says, the statistician would not be using strictly random numbers. Most statisticians would prevent this difficulty from becoming serious by being careful not to be too fussy, i.e. they would reject only very peculiar subsequences. Therefore it seems unlikely that the difficulty will cause many sleepless nights for statisticians.

But suppose that the statistician is very fussy. Then, says Spencer Brown in effect, he will have so many criteria of prefect randomness that he will reject nearly all subsequences. The author's argument here is that if each test lets through 95 per cent of the subsequences that are still left, then only a proportion $(0{\cdot}95)^n$ will be left in after n tests, and if n is large the proportion will be negligible or zero.

The natural answer to this argument is that the criteria, for subsequences of a fixed length, should first be roughly ordered in ascending order of complication. The significance levels for reasonable rejection will then not all be 0·05, but will form a sequence $P_1, P_2, P_3, \ldots$, where $P_1 \geqslant P_2 \geqslant P_3 \geqslant \ldots$. It will then be possible to have a large set of tests and yet not to reject a high proportion of subsequences of the given length. The usual applications of random numbers in statistics will then be approximately valid.

Apart from this theoretical objection to the use of random digits the author believes that no method of producing perfectly random digits is satisfactory and that this throws doubt on statistical experiments, especially on those designed to test E.S.P. (extra-sensory perception). This objection is not supposed to be of the kind that no physical probability can be 0·5 correct to an infinite number of places of decimals, which would be a trivial objection ; it seems to be rather that good reasons for rejecting the perfect randomness can always be found without a very great expenditure of effort. This view is published at a time when it should not be hard to disprove, since methods of producing random numbers are at present becoming very efficient with the rapid advance in electronics.

The author gives some examples of significant deviations from randomness in the tables of Fisher and Yates, of Tippett, and of Kendall and Babington Smith. The highest significance level reached is 1/7340, which is hardly comparable with the best results in E.S.P. experiments. For example, Soal and Bateman[1] summarise a sequence of experiments that reach a significance level of about 10^{-70}. The author's reply to this would be (p. 87) 'p-values [tail-area probabilities] like 10^{-35}, which are so much prized in a certain type of investigation, cannot possibly give us greater certainty than, at the outside, a p-value of 10^{-3}; and a p-value of no less than 10^{-2} serves in nearly all cases as the most stringent practicable criterion, since if an experiment is at all difficult we are not likely to do it more than 100 times.' It seems to the reviewer that a tail-area probability of 10^{-35} is in an entirely different class from one of 10^{-3}, irrespective of whether the experiment is to be repeated. It would be easy to reach a significance level of 10^{-3} by making say 100,000 independent tests for significance. (This would be practicable on a modern computer, or manually, if enough people became interested.) But to obtain a level of 10^{-35} for a 'finite random sequence', which had already passed several of the simpler tests, would be very difficult, except by means of tests constructed artificially after looking at the sequence.

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[1] *Modern Experiments in Telepathy*, Faber and Faber, London, 1954, pp. 425, p. 311

If the reputation of the theory of probability is at stake it is to be hoped that one of the American research corporations will decide to produce a fully reliable randomiser, and will publish some of its output without any editing. If 1,000 readers of Spencer Brown's book perform tests on this output, then of course some of them may get tail-area probabilities in the region of 10^{-3}. Unfortunately there is then a danger that only the significant results will be announced! Therefore it would be better if a set of tests could be performed by the publishing corporation and the results of all of them published with the random numbers. When this is done it would be important that the serial test, first described by Kendall and Babington Smith, should be given its correct interpretation, as found by the reviewer.[1]

If the author turns out to be right that no proper randomiser can be produced then the reviewer would regard the result not as a contribution to the theory of probability but rather to theoretical physics, or to psychokinetics.

A difficulty concerning orthodox statistics is mentioned on page 65. Fisher is quoted as saying that (*a*) no isolated experiment, however significant in itself, can suffice for the experimental demonstration of any natural phenomenon, (*b*) any series of experiments may be regarded, for the purpose of statistical argument, as a single or isolated experiment. The author draws the obvious conclusion and describes it as a 'beautiful paradox'. On looking up the original reference[2] we find that Fisher went on to say that 'what is really wanted is a reliable method of procedure' and not just an isolated record. Nobody would quarrel with this as a *desideratum* but surely a tail-area probability of 10^{-35} cannot be ascribed to chance. The

[1] 'The serial test for sampling numbers and other tests for randomness', *Proc. Cam. Phil. Soc.*, 1953, **49**, 276–284

[2] *The Design of Experiments*, 1949, pp. 13-14

reason why many statisticians are unconvinced by such tail-area probabilities in E.S.P. experiments is that they fear that the experiments may not have been properly performed or reported, and the reason why they pick on E.S.P. for their suspicions is that they regard the initial probability as small that there is anything in it. The general belief in E.S.P. would be increased more if new experimenters were to obtain significant results than if the same old experimenters were to continue to slash their previous tail-area probabilities.

Apart from the criticisms of probability and statistics, the book contains a few chapters on other philosophical questions. For example, Chapter 4 makes a point made by vonWright previously,[1] a point that may be put in a nutshell as follows :

> A meson is lighter than a proton.
> What is the reason ?
> For if it were heavier, none dare call it meson.

On page 83 there is a reference to 'Wisdom's Cow'; a cow generally has a number of properties no one of which is essential to its cowness. It is surprising that the paradox connecting the meson and the cow is not mentioned, in view of the author's love of paradoxes. The paradox can be resolved by the following definition of definitions of things : Let $Q_1, Q_2, Q_3, \ldots$ be a sequence of qualities. Let $x_i = 1$ or 0 depending on whether anything, T, has quality Q_i. Then T is a meson (or cow) if $f_j(x_1, x_2, x_3, \ldots) > 0$ $(j = 1, 2, \ldots)$, where the functions f_j define a meson (or cow). Often a single linear or quadratic form would be adequate. This definition of definitions of things is analogous to the use of discriminant functions in statistics.

On page 19 it is argued that the 'Law of Uniformity cannot be falsified ; for when our observations are not uniform we say we are not observing nature'. The reviewer thinks that we should usually prefer to say that the 'circumstances' were different or that the relevant laws of nature were only probabilistic.

I. J. Good

Thomas of Bradwardine His Tractatus de Proportionibus.
Edited and translated by H. Lamar Crosby, Jr.
The University of Wisconsin Press, Madison, 1955. Pp. xi + 203. $3.50

The rehabilitation of medieval mechanics which was begun early this century by Pierre Duhem in Bordeaux and has recently been furthered with great distinction by Anneliese Meier in Rome, is now being continued in the United States, notably by the publications of the University of Wisconsin

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[1] *A Treatise on Induction and Probability*, Routledge and Kegan Paul, 1951, pp. 141-146

Press. Under the editorship of Marshall Clagett the texts and translations of key works are becoming generally available.

Outstanding among the fourteenth-century contributions to medieval kinematics and dynamics was the treatise on the ' proportions of the speeds of motion ' written in 1328 by Thomas of Bradwardine before he left Merton College, Oxford, to become successively Chancellor of St Paul's, Chaplain-Confessor to Edward III (in whose service he saw much action in the field, e.g. Creçy), Archdeacon of Norwich and finally in 1349 Archbishop of Canterbury. This office he held for barely a month before he succumbed, like so many other great men of the time, to the Black Death.

Even in his own day, despite his ecclesiastical eminence, Bradwardine was esteemed primarily for his intellectual achievements and was dubbed ' Doctor Profundus '. His short tract on ' proportions '—or, as we should now say, ' functions '—is of outstanding importance in the history of mathematical physics because it appears to have been the first attempt to analyse physical phenomena in terms of more sophisticated mathematical relations than simple linear proportions. Consequently, although the particular relations considered by Bradwardine are now regarded as inappropriate for the purpose intended and moreover his entire discussion is purely *a priori* without any appeal to experiment, we must salute him as one of the great pioneers of modern scientific method. Despite its ultimate complete supersession in the seventeenth century, his tract was of great influence on subsequent generations, as is evident both from the comparatively large number of manuscript copies which still survive and from the fact that it was printed in many editions in the late fifteenth and early sixteenth centuries in such places as Paris, Venice, Vienna, Valencia, and Wittenberg. In view of the complete absence of Bradwardine's theological writings from the early printed editions of his works, Dr Crosby concludes that it would seem that his work in natural philosophy was of considerably greater interest than his theological writings to men at the beginning of the sixteenth century.

Dr Crosby's scholarly book must be welcome first and foremost because it makes easily available for students both the original Latin and an excellent English translation of the *De Proportionibus.* The text is based on a collation of four fourteenth-century manuscript versions, three other less reliable manuscripts being used for auxiliary checking. The text and translation are printed on alternate pages facing each other. Variant readings and a most useful series of notes are printed at the end. The Introduction, which runs to nearly sixty pages, comprises a short biography of Bradwardine, an annotated list of editions and manuscript sources, a short essay on the general significance of the tract and a lengthy detailed critical analysis.

The central problem of Bradwardine's tract concerns the relation of dynamics to kinematics. His theory of the relation of motive force F, resistance R and velocity V can be expressed, *in modern notation*, by the formula $V = \log_n (F/R)$. This peculiar relation, which he advanced in preference to the Aristotelian simple F/R proportionality, was the result of his struggle to produce a satisfactory formula in the absence of any clear recognition of the distinction between inertial and other resistances to motion, e.g. frictional, a problem which was only finally resolved with Newton's explicit formulation of the concept of mass. The origin of Bradwardine's formula may be briefly summarised as follows. It was realised by Aristotle himself that his law, V proportional to F/R, cannot apply when, as often happens, F is insufficient to cause motion. Several medieval writers made suggestions for overcoming this difficulty. Bradwardine restricted his attention to cases where motion does occur and also interpreted Aristotle's law of proportionality in a peculiar sense suggested to him by the rule given in a commentary on Euclid that, if $a/b = b/c$, then $a/c = (a/b)^2$. He argued that, if V is produced by F/R, then $2V$ will be produced not by $2F/R$ but by $(F/R)^2$.

Particularly noteworthy in Bradwardine's analysis is his great advance on Aristotle in regarding velocity as an instantaneous 'quality' of motion and not merely as a simple quotient obtained by dividing distance by time.

The great defect of Bradwardine's work, when judged irrespective of his neglect of empirical considerations but on its own terms, is the extreme poverty of his mathematical notation. Indeed, it would seem that this deficiency, which was characteristic of the fourteenth and fifteenth centuries no less than the absence of a proper appreciation of the interrelation of theory and experiment, delayed the rise of classical dynamics for several centuries. (The publication in 1638 of Galileo's *Two New Sciences* comes almost midway in time between Bradwardine's tract and the present day.) The development of key kinematic ideas, such as acceleration, became scholastic achievements of extraordinary difficulty and subtlety. In the development of such ideas the Merton school of the fourteenth century, of which Bradwardine was an outstanding member, played a vital rôle grossly underestimated by Duhem in his enthusiasm for the great fourteenth-century French school of Buridan, Oresme, etc. Nevertheless, Crosby seems to overshoot the mark when he claims that Bradwardine's pupil John of Dumbleton interpreted his master's formulation of dynamics in a manner akin to Newton's second law of motion.

Although, in the present very incomplete state of our knowledge of fourteenth-century dynamics, only a handful of experts can even begin to adjudicate on the questions at issue, the crucial significance of that century in the evolution of scientific method makes it highly desirable that books such as Dr Crosby's should be as widely read as possible by philosophers of science no less than by historians.

G. J. Whitrow

Existential Psychoanalysis. By Jean-Paul Sartre.
Philosophical Library, New York, 1953. Pp. viii + 275. $4.75

This is a translation of some final chapters of *L'Être et le néant.* It is a contribution both to psychology and to the philosophy of psychology ; in addition it contains metaphysic. It seems to me important.

It is better, according to Sartre, to define a man by his aims rather than by his desires ; then what we want to understand is not the choice of an aim in general, but why this particular man has this particular aim, and this involves finding his ' original project '. Every other project, such as going rowing, is a manifestation of the original project in particular circumstances.

The original project is the aim simply of being—' man is the desire to be ' (p. 63). At this point arises a distinction between ' being-in-itself ' and ' being-for-itself ' (terms that will hardly endear themselves to the English-speaking world). ' Being-for-itself ' is the reality a man lacks, what he has not become, and to the extent that he lacks it he lacks being. His project is to overcome this lack. ' Existential psychoanalysis '

> is a method destined to bring to light, in a strictly objective form, the subjective choice by which each living person makes himself a person ; that is, makes known to himself what he is. Since what the method seeks is a *choice of being* at the same time as a *being*, it must reduce particular behavior patterns to fundamental relations—not of sexuality or of will to power, but of *being*—which are expressed in this behaviour (p. 87).

Sartre turns to show that there are three fundamental categories, *to make, to have, and to be* ; on examination, however, he finds that the first two reduce to the third (later on he mentions that there is never being without having, but being in some way remains more fundamental).

En passant there are several striking comments, where he discusses the ' digested indigestible ', finds that ' desire is the desire of devouring ', shows that the serious man is in ' bad faith '. He proceeds to locate the fundamental form of desire in appropriation—pointing out that the reason why some peoples buried their dead with their belongings was not because of a belief that the dead man could use these things but simply because they belonged to him, in the sense of forming part of him. Thus he analyses possession in terms of being united with an object considered as appropriated ; the desire for a certain thing is not just the desire for it, but the desire to be united with it (which shows that ' appropriation ' is not just ' owning ' but incorporated into one's being). In this way he shows that *to have* is reducible to a relation of *being*. Again, he claims that a gift is a ' primitive form of destruction ' (p. 141).

> If then existential psychoanalysis encounters evidence of *generosity* in a subject, it must search further for his original project and ask why the subject has chosen to appropriate by destruction rather than by creation. The answer to this

> question will discover that original relation to being which constitutes the *person* who is being studied (p. 143).

In general,

> The goal of existential psychoanalysis is to rediscover through . . . empirical, concrete projects the original mode in which each man has chosen his being (p. 155).

Sartre next proceeds to a ' psychoanalysis of things ', which the well known philosopher of science, Bachelard, tried to do, we are told, for *water*. This, one may intervene to say, is not so fantastic as it may seem. Sartre proceeds to his famous analysis of the *slimy* (pp. 168-89), which consists of a phenomenological—and in his view objective—description of all kinds of facets of the experience of the slimy. It is with this form of analysis that Sartre is at his best. The point of this phenomenological analysis is that it reveals the *being* of the slimy. So all things, human and inanimate, have their being in modes of being.

Finally Sartre enters upon the question of ' bad faith '. ' Bad faith ' is ambivalence towards oneself. It is not, of course, to be identified with lying, for in bad faith one is not aware of the false note—and yet one is not wholly unaware either, one somehow knows that one is taking oneself in. And it is not this duplicity towards another but towards oneself that constitutes bad faith. Sartre gives fine descriptions of instances, such as that of a woman who keeps from herself the admission that she is going to be seduced but whose actions made no sense unless they lead up to this. A different sort of example is that of a waiter, who must if he is to satisfy his clientèle play the part of being a waiter, which is not the same thing as carrying out the acts that constitute waiting—he is therefore what he is not, in the Hegelian language of Sartre. Sartre concludes that there is an inherent conflict in bad faith : it seeks ' to flee what it cannot flee, what it is ' (p. 268) ; and there is even bad faith in the original project of bad faith, for it could not be adopted with sincerity (p. 261).

Though not entirely plain, I think the overall point here is that bad faith or the ambivalence that constitutes it either prevents a man from achieving being or is a manifestation of failure to be.

I do not know what readers will make of this. I would suggest it would be worth their while getting the text and struggling with it. M. Sartre has come to be known in this country for two unfortunate things : the author of obscure or misleading saws, such as ' man fundamentally is the desire to be God ' (p. 67) or ' man is a useless passion ' (p. 199), which are very fine when understood as aphorisms displaying what they epitomise ; and the purveyor of bad—even appalling—logic.

It is quite true that Sartre has expounded some of his thought in a form that passed for logic, and very bad logic it is, though not so gross as it is sometimes represented. But there is no necessity whatever to expound his views in this way (though no doubt if we are to apply the ideas of the present book we shall be reaching the being of Jean-Paul if we study him in his chosen medium of quasi-logic, and otherwise we shall be getting existentialism without a lacing of Jean-Paul) ; and I wish to suggest that the content, once the logical trappings have been lost in oblivion, is important. I do not know whether the pseudo-logical guise appeals to the illogical nature of the logic-loving French, but it has probably done the author undeserved harm in England where the accurate use of logic is something of a fetish. M. Sartre is surely not only an outstanding French philosopher, and one would perhaps have to go back some centuries to find a French philosopher to equal him, but he has certain qualities that put him almost alone in the world of contemporary philosophy : he is thoughtful, has something to say, he is interesting, and what he has to say gives the impression that it matters.

Sartre's theme is man—or rather this particular man *vis-à-vis* other men. He has a theory about their interrelations which is a theory of being and the free act. His theory of existentialism might be put like this :—Confronted with a situation in which a decision must be taken and an action carried out, yet lacking sufficient causal factors derived from convention, or moral or religious codes, to determine the decision or the action (for to be determined by these would be to have chosen bad faith as a basic principle of life), then the person involved, without motive for decision or action, experiences a chasm between him and the decision and action that must be taken, which gives rise to the experience of anxiety ; but at that very moment he is free to decide and act, and his free move is a leap across the chasm, abolishing it, taken in accordance with his fundamental choice of project as the basic form in which he desires to be. (The free leap is, it is true, determined by the project, but though it could be interpreted as a motive the project is not a motive in the ordinary sense.) Though not devoid of methodological difficulties, this seems to me to make very good sense—at least on the phenomenological level, as most of it is.

It is perhaps a pity that Sartre calls his work ' existential '. No doubt the reasons are historical and connected with his queer logic about existence, and there is the good point that his philosophy is concerned to emphasise the immanent. Still ' existential ' is not a very graphic word for this. It is also a pity he used the word ' psychoanalysis ', for his practical procedures seem to bear almost no relation to psycho-analysis. The reason no doubt is a certain kinship between his theory and the theory of psycho-analysis. The nearest I can get to an adequate phrase is Psychogenetic Phenomenology.

It is not surprising therefore that he discusses Freud in some detail. He outlines his very considerable agreement with and his very considerable divergence from psycho-analysis. His interesting criticisms of it appear novel and may well prove important. This would take some sifting, because his excellent presentation of Freud's work contains one or two slight—very slight—errors ; but to investigate this would require a paper to itself. This theme is very much philosophy of science.

I should like to end by emphasising that this, though a book on philosophy of science and on philosophy, is largely a book on psychology. This alone gives it a place for our attention, since so few books on psychology have ever been written.

J. O. Wisdom

ABSTRACTS

Dialectica, 1956, **10**, No. 1

F. Gonseth, 'Dialectica entre dans sa dixième année d'existence'

The original intention was to produce an organ for 'open philosophy'. This start has been maintained in spirit though not adhered to with complete rigour.

N. Rotenstreich, 'Kant's Schematism in its Context'

The article attempts to analyse the position of schematism in Kant's theory of empirical knowledge. The view suggested in the article is that it is the idea of schematism and not the idea of transcendental deduction which is Kant's real reply to Hume's question. Empirical knowledge according to Kant is possible because time itself, being the mediating link between concepts and data, contains a minimum of order and hence a minimum of rationality.

H. Binder, 'Der Begriff des Normalen in der Psychiatrie'

The criterion of the mentally normal is that the psychic disposition, which always tends to maintain or restore the balance of the various psychic functions, is unimpaired. Since this striving for balance can assert itself throughout the psyche despite the existence of partial disharmonies and conflicts, the latter still form part of normal life. In all non-normal subjects, the inner tendency towards psychic balance is impaired and this causes fixed disturbances of the inner equilibrium ; this reveals itself in a consistently displayed inadequacy of behaviour.

E. Simenauer, 'Betrachtungen über die biologischen Grundlagen des Denkens und des Willens'

The philosophy of perception and knowledge has lately been confronted with the establishment of the psychological basis of ideation. Contemporary attempts at correlating the concepts of depth-psychology to neuro-physiological theories are broadening the biological approach to enquiries which have been traditionally reserved to philosophy. In this preliminary and programmatic communication reference is being made to one of these neuro-physiological theories (W. Scheidt) which endeavours to describe conscious and unconscious thinking in terms of biological processes taking place in the various neuro-fibrillary systems. Thus a biological theory of knowledge is emerging which holds out hope of fruitful co-operation between philosophy and science.

M. Gex, 'Chronique de caractérologie. La caractérologie de René La Senne'

René La Senne has made a school of characterology of the Dutch writers Heymans and Wiersma, known throughout France. This purely psychological characterology composes types of character, utilising three basic factors, emotivity, activity, and reverberation which allows one to assign to each type a description analogous to that of a chemically composed substance. From varied studies, ranging from literary history to psycho-sociology, show the fertility and flexibility of this method of analysis of character.

H. Dingler, ' Geometrie und Wirklichkeit (Forts.) '

This continues the article in the previous Number.

Dialectica, 1956, **10**, No. 2

F. Gonseth, ' De l'humanisation de la technique '

The world of technicians sometimes takes note of its powers and responsibilities. A great problem opens up which may be analysed more profoundly by introducing the concept of feed-back from cybernetics. The author also discusses certain other conditions inseparable from the intention to humanise technique.

Y. Reenpaa, ' Versuch über die Beobachtungsgrundlagen der exakten empirischen Wissenschaften '

This is an attempt to explain certain difficulties in the philosophical understanding of the natural sciences by examining in one definite case (the so-called relation of uncertainty) the structural problem of an aprioristic observation and by advancing from this starting-point to the conceptual side of the problem.

The special case which the writer tries to examine thoroughly is that of visual minimum perception. The conclusion is as follows : the so-called full minimum perception (minimale Vollbeobachtung), i.e. a perception which is minimal in its duration, area, and intensity and which is, in its colour quality, determined, can be depicted in the *g*-cm-sec-system as a quantity of the dimension of the Wirkungsgrösse (dimension g cm^2 sec^1). By calculating the ' Wirkung ' in question we get a value of 1 to 8 quanta. This result is compared with those given in papers by Hecht, Shlaer and Pirenne and by Bouman and Baumgardt concerning the minimum number of light quanta necessary for visual excitation.

Finally it should be mentioned that the conceptual representation of the minimum perception includes the question of uncertainty, because this perception is a simultaneous or coincidental perception.

A. Pap, ' Logical Empiricism and Rationalism '

According to the version of logical empiricism which Louis Rougier advocates (*Traité de la connaissance*) in opposition to ' *a priori* rationalism ', necessary truths are merely the consequences of freely adopted linguistic conventions. Rougier attempts to substantiate this ' conventionalism ' by reference to pluri-valued logics in which the laws of two-valued logic allegedly do not hold. But he confuses *sentences* and *propositions :* what depends on conventions is only that a given sentence expresses a necessary proposition, not that the proposition expressed is necessary. It still does not make sense to say that logical truths are arbitrary postulates or consequences of such, for what is called a logical truth is not a formal postulate but the *proposition* that whatever interpretation satisfies such and such formal postulates also satisfies such and such formal theorems.

Rougier's theory of empirical knowledge corresponds, in rough outline, to the phenomenalism and unduly stringent (even self-contradictory) verifiability principle of meaning which dominated an early phase of logical empiricism, and is therefore outdated. Moreover, his exposition of early doctrines of the Vienna Circle is infected with logical blunders, such as the confusion of propositional functions and universal propositions. His critique of metaphysical pseudo-statements and pseudo-problems that arise from the imperfections of the syntax of ordinary language, is likewise outdated as compared with recent linguistic analysis in England.

F. de la Horbe. ' Sociologie et philosophie : Hommage de la sociologie à l'idéonisme '

The history of theories displays oscillation between the two poles of belief and experience. The development of the exact sciences is based on the predominant influence of the latter while moral and social theories remain more or less prisoners of philosophy. It was only in the nineteenth century that sociological theories tended to become scientific. This article explores the philosophical basis of sociology. This composite science offers a spectacle of scientific analysis being replaced by verbal classifications. In this difficult phase ideonism opens the way towards a doctrine favourable to elementary truths.